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CONFABULATION IN THE WERNICKE-KORSAKOFF SYNDROME[1]

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Confabulation, according to the textbooks, is one of the characteristic symptoms of the Korsakoff syndrome. Indeed, the disorder is frequently referred to as the "amnesic—confabulatory syndrome." This nomenclature would imply that the two symptoms—amnesia and confabulation—are somehow interlocked and the joint defining attributes of the disease, present in all its stages.

A definition which requires the conjunction of both symptoms is logically defensible, but in fact has not been adopted in clinical practice. Remarks recurring in the literature point to the conclusion that confabulatory behavior cannot be detected in every patient whose gross anterograde and retrograde amnesia indicate the diagnosis of Korsakoff's syndrome. Indeed, in the chronic stage of the disease confabulation is the exception rather than the rule, even though it may have been present at the beginning of the illness. Korsakoff (20) was but the first in a line of investigators who presented confabulation as a contingent rather than a necessary ingredient of the amnesic symptom complex. Likewise, Kräpelin's (22) observation that this behavior was prominent in the early phase of the disease and subsequently disappeared, has been reaffirmed, though it has also been neglected by others.

Some of the uncertainty must be attributed to the ambiguity of the term itself, so that Bender *et al.* (3), for example, could describe as confabulation an instance of pictorial reproduction which distorted meaningless patterns to look like familiar and meaningful symbols. There is no compelling reason for stretching the meaning of the term beyond the confines of verbal behavior. Its reference within that area is quite confusing enough. The dictionary will not help in clarifying its meaning, and the apparent unanimity of the textbooks is deceptive. Bleuler (6) meant by confabulation "free inventions taken as experiences"; for Cameron and Magaret (8) it implies "making up of a likely story"; Henderson and Gillespie (16) list it as that sub-category of pseudo-reminiscence which involves fabrication. From these definitions it would appear that the content of confabulation is invented, fabricated, rather than reproduced or reconstructed from actual data; yet the clinical examples quoted are overwhelmingly of the reproductive or reconstructive type. Henderson and Gillespie indeed so related the term to paramnesia as to include distortions of true events as well as the invention of those which never took place. Korsakoff (21) quite explicitly traced the pseudo-reminiscences of his patients to true memories. If

[1] The report is based on studies of two groups of patients: 1) those with a diagnosis of Wernicke's encephalopathy, and 2) those diagnosed as having Korsakoff's disease or "alcoholic psychosis," and with a previous record of Wernicke's encephalopathy. The title is not intended to express a position on the identity of the two syndromes; it merely recognizes the fact that a large proportion of Korsakoff patients had gone through Wernicke's disease, and that after their recovery from the latter many patients are left with the amnesic disorder associated with Korsakoff's name.

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one tabulates the many uses to which confabulation has been put in the literature, it appears to refer to anything from a deliberate lie or a product of creative imagination (neither of which is characteristic of Korsakoff patients), to the rounding out of a memory of something experienced, learned, or casually observed—in fact normal remembering.

DEFINING CONFABULATION

Confabulation is a false verbal statement about facts. Should the term apply to any incorrect statement about any set of facts, include purposeful deceptions and fantastic delusions as well as misstatements arising from confusion? If it is to refer to a behavior disorder characteristic of the Wernicke-Korsakoff syndrome, then a more restricted definition is required, one moreover which differs from the textbooks quoted. In order to arrive at a definition satisfactory for this purpose, the following properties of confabulation must be considered.

1. Deliberate falsification;
2. Imaginative elaboration and distortion;
3. Derivation of content from actual personal experience and other true information;
4. Autobiographical reference of content;
5. Verisimilitude, coherence, stability.

Surveying the literature, one finds that on none of the five scores has the term been consistently defined. Perhaps in order to admit the full variety of anomalies observed in the verbal behavior of Korsakoff patients, yet without multiplying the technical terms, confabulation has been used to describe behavior both high and low, positive and negative on the criteria listed above. For Régis (30), who discussed the term in the context of presbyophrenia, confabulation referred to 1) actual memories which the patient distorts and projects from the past into the present; 2) the figments of dream-like states; and 3) imaginative fabulation or *mythomania*. Moll (25) who wrote about the amnestic or Korsakoff syndrome, and as a matter of fact used the term "fabrication," bracketed five types of verbal behavior under the headings: 1) random fabrications to fill out gaps with "*ad hoc* concoctions"; 2) distorted recollections or pseudo-reminiscences; 3) true recollections misoriented in place or time; 4) fabrications based on hallucinations; 5) remnants of dreams, not recognized as such. Subsequent writers may have been less catholic in their selection or less systematic in their classification, yet most of them have meant by confabulation all the varieties of fabrication enumerated by Moll. Reports on their patients, on the other hand, rarely cover this entire range; clearly there is no close association between the amnesic psychopathology and false statements drawing on all five sources listed by Moll.

DELIBERATE FALSIFICATION

Ever since Bonhöffer (7) distinguished confabulations of exigency from spontaneous tales of adventure, the view has gained currency that amnesic patients invent or transpose material in order to fill gaps caused by the absence of appropriate memories. Pick (27) in an early paper on confabulation reaffirmed Bonhöffer's distinction, and further pointed out the contribution of the patient's suggestibility, the ease with which his communications can be steered by leading questions. Though the teleological closure implicit in the gap filling formula has appealed to many other students, none has maintained that confabulation is a deliberate and conscious attempt to avoid a dilemma, to escape the embarrassment (*Verlegenheitskonfabulation*!) of not remembering that which any sane person ought to remember.

On occasion there is good reason for suspecting a deliberate lie or substitution, yet even that is more likely to take the shape of a negative rather than a positive reply. Korsakoff patients do at times evade an answer with a remark which sounds so contrived that it seems unlikely that they themselves are entirely unconscious of it:

falseness. An example would be when a patient is asked to name the book he has been reading or say something about its contents, and he replies "I wouldn't know, I am not interested in that sort of thing." Instead of an evasive response one may even get an instance of "*ad hoc* concoction," but these are not typical examples of confabulation, and the possibility that the false account has arisen from some confusion can never be entirely ruled out.

French neuropsychiatrists have thought it desirable to distinguish confabulation, even while leaving it otherwise loosely defined, from *mythomania*, a pathological tendency to fabricate stories. Régis, though he minimized its contribution, nevertheless included the latter in the genus of confabulation. Janet (18), however, distinguished the two symptoms; confabulation for him implied that the person who engaged in it neither cared nor considered that he was telling an untruth. Delay (10), Bernard (4) and Flament (12), though otherwise their definitions may differ, made the same distinction, implying that confabulation does not involve and indeed fundamentally differs from deliberate lying. It seems reasonable to follow them in excluding falsification by intent from any definition of confabulation.

IMAGINATIVE ELABORATION

Every act of remembering is a creative as well as a reproductive process. Bartlett (2) has analyzed the mechanisms of this process in normal remembering, and Lidz (23) had drawn attention to the relevance of Bartlett's theory to the confabulatory behavior of amnesic patients. The question to ask, then, is whether confabulation in the Korsakoff syndrome involves a creative or imaginative feat over and above that which goes into normal remembering. Does Bonhöffer's distinction between passive confabulation of exigency and productive or fantastic confabulation define two independent symptoms, two different mechanisms? Many students of the Korsakoff syndrome have found it difficult to admit the existence of productive fabulations; it seems too paradoxical that patients who display such an unusual lack of spontaneity should habitually blossom forth into acts of creativity.

Van der Horst (17) and Körner (19) dismissed the paradox: that which might appear as creative fantasy—they argued—in fact originated in passive responsiveness. This is true also of the psychoanalytic point of view stated by Hartmann (15), who saw in confabulation the manifestation of those autonomous processes which take place in every human mind, but do so with less restraint amidst the deficit characteristic of the amnesic patient's function. Against the mechanisms familiar from psychoanalytic theory, Ploog (29) referred to principles of Gestalt psychology which come into operation through the indirection characteristic of the amnesic syndrome. Korsakoff's (21) remark about the frequency of the themes of death and funerals in pseudoreminiscences, and his suggestion that these presumably represent associations in the unconscious sphere, could be taken as an anticipation of the conclusion Betlheim and Hartmann (5) drew from their experiment, namely, that distortions in the report of amnesic patients follow the rules of dream symbolism. More probably, Korsakoff's observation which, incidentally, has not been confirmed in subsequent studies, illustrates the stereotypy rather than the symbolic content in confabulation. Bernard explicitly denied the operation of symbolism in the confabulations of amnesic patients, and contrasted it with the archaic quality of schizophrenic delusions. Our own studies have produced no evidence contrary to Bernard's view.

The typical approach to the problem of confabulation thus appears to be that of explaining its occurrence from the presence of the other, more especially the amnesic, disturbances of the amnesic syndrome. A difficulty arises from the observation that it is not a reliable concomitant of those other

symptoms. Lidz attributed its occurrence, in part at least, to an effort to avoid anxiety, Williams and Rupp (38) to tendencies inherent in the patient's personality structure which is usually not radically affected by the Korsakoff syndrome. They thought that the confabulating patient combines the extremes of introversion and extraversion, by which they meant uncommunicativeness, at one end, superficial sociability and realism at the other. This is not self-evidently the most imaginative, the most creative person. Neither, for that matter, is the latter the most promising candidate for florid dreams or delusions. In a sense, no doubt, confabulation and dreams and delusions share something with artistic creation, but for all that they need not be regarded as examples of productive imagination which also entails direction, discipline and spontaneity. It would appear that the Korsakoff patient neither sets the direction of nor imposes discipline upon his confabulations. The question of his spontaneity is more debatable.

In this connection, Bernard once again adopted the extreme conservative position, by stating that Korsakoff patients confabulate only in response to leading questions. Pick (28) had implied much the same in his comments on confabulation of exigency. The literature, however, contains too many exceptions to this rule to ignore some measure of spontaneity in the genesis of certain pseudo-reminiscences. Angelergues (1) put the case more cautiously, presenting this as the typical example but admitting the propensity of some Korsakoff patients to elaborate their paramnesic themes into fluent confabulations, and attributing such tendencies to their temperament. The trait of loquacity is hardly consistent with the general picture of the apathetic, indifferent, taciturn chronic Korsakoff patient; it is unlikely to be a symptom of the disease but may indeed persist alongside its symptoms. Spontaneous production, therefore, must be admitted into our definition of confabulation, though not as a necessary or indeed a typical condition of its occurrence. Furthermore, spontaneity of production must not be confused with creative imagination: the evidence is insufficient for treating productive confabulation as a symptom different in its mechanism or its purpose from the examples of paramnesia which arise in reply to straight questions. At the same time, the desire to please the interviewer, to avoid embarrassment, to fill in gaps, is rarely the determining causes of confabulation. In a very wide sense confabulation always fills gaps, but not in the sense that the patient is aware of those gaps. When he is aware of the gap he usually answers with a plea of ignorance.

SOURCE OF CONTENT

Williams and Rupp concluded that all confabulation consists of actual incidents in the patient's life. Similar views, though perhaps not always so categorically, have been expressed by other writers. No doubt, most of the confabulatory material either patently originates in a displaced or otherwise distorted personal experience or could be of that derivation. Nevertheless, it may be wrong to attribute even all the autobiographical data to that source. Mönkemöller's (24) comments on an alcoholic Korsakoff patient's elaborate account of his medical history are of interest in this connection. Since the patient had no record of previous illness, his stories about his experiences in a number of other hospitals and of his interviews with physicians could not stem from personal recollection. The case is an example of a Korsakoff patient drawing on second-hand information and weaving that into his own life history.

Quite another problem is posed by instances of fantastic confabulation which involve the supernatural, the clearly fictitious or the historic past. In the early phase of the disease many of the patients are delirious and their erratic flights into the realm of fancy can be credited to that condition

Instances of this type of talk, however, have been observed in the chronic stage as well. Flament, whose paper dealt with the post-traumatic Korsakoff syndrome, reported that in 18 out of 20 patients fabulation contained nothing fantastic or supernatural, and retained the character of reality. This still leaves ten per cent to form an exception to the rule. Clarke (9) was only one of the clinicians to point out that Korsakoff patients may fill in amnesic gaps with bizarre and fanciful stories, imaginary experiences and attainments of their own. Although he also remarked that these falsifications of memories and confabulations disappear upon the patient's gaining insight, this qualification would not exclude them automatically from the post-delirious stages of the disease, and indeed examples of autobiographic information more appropriate to the patient's ideal than actual self are not uncommon in the chronic phase. It is possible to deal with these by including among the pre-morbid memories the patient draws on, his wishful fantasies and imaginary horrors as well as his real experiences.

Another solution has been proposed by Williams and Rupp, who distinguish confabulation from fabrication, assigning all the fantastic content to the latter. Importation from outside the patient's personal experiences evidently does occur, and invention drawing on no other source than his momentary inspiration cannot be ruled out. In order to discover the mechanisms of confabulation it seems desirable to distinguish the distortion, condensation, elaboration, and displacement of genuine memories from the incorporation of material culled from the pages of history, legend, fiction and the biographies of contemporaries, and both from the figments of imagination. Unfortunately, the patients' behavior does not respect these considerations, for importation of themes and invention rarely occur without some admixture of simple paramnesias. Confabulation, in its widest definition, almost invariably includes some kernel of genuine experience, and often the sorting out of one source of its content from another becomes a detective task insoluble with the information obtainable. Nevertheless, it seems advisable to restrict confabulation to the reproduction of personal memories outside their actual context or to their distortion within realistic limits, and to apply a second term, fabrication, to the more fantastic and incongruous material.

AUTOBIOGRAPHICAL CONTENT

Most of the data on confabulation have been collected in clinical interviews, and these tend to focus on the patient. Consequently, the view has gained currency that all confabulation consists of the patient's inaccurate reports on himself. The definition proposed in the previous section tends to restrict the term in the same direction. If confabulation draws on actual incidents in the patient's past, displacing them in their temporal sequence, or in their social and geographical context, then its area of reference is principally, even though not exclusively, the self. Of course, a patient's report about his father's activities yesterday, based on events of several years ago, would be just as confabulatory as his detailed description of his own work situation in reply to a question about what he had been doing that morning. The same mechanism would also be operating when he attributes the entry of the U.S.A. into the Second World War to Hitler's submarine warfare, or names Pershing along with Patton as outstanding generals in that war.

Confabulation, according to our definition, would typically contain autobiographical reference, but would neither be restricted to that, nor would that content be a sufficient criterion for classifying a non-veridical narrative under the heading of confabulation.

VERISIMILITUDE, COHERENCE, STABILITY

The question of verisimilitude has been largely dealt with by the distinction proposed between confabulation and fabrication. The

impossible and the inherently improbable have been relegated to the latter class, yet it should be stressed that the verisimilitude of confabulation is often a relative one. As one listens to the patient talking, it often sounds as if it may all have been just like that, but in fact could not have been so: not at the time or place he is referring to. In other situations, however, the story appears deceptively convincing, in its setting as well as its general plausibility. The tone of conviction is no reliable measure of veridicality; the patient's rocklike certitude, as Kräpelin remarked, can be very misleading.

Kräpelin was also impressed with the precision and wealth of detail characteristic of confabulations. These, however, are not universal characteristics; the content, even though coherent, is more often blurred in outline, and frequently couched in general terms where normal discourse would be specific. This quality as well as the imperfect articulation of any one unit of content with the preceding themes justifies Scheid's (31) comparison of the Korsakoff patients' reminiscences with dream memories. Confabulations vary widely in their internal coherence: among patients, according to the segment of discourse examined, and the stage of the disease process. Undoubtedly, the patient's communications gain in coherence as he progresses from the acute to the chronic phase, though complete incongruity between two or more serially presented themes may be found at every stage. In the acute phase totally incoherent talk ought probably to be attributed to delirious fantasy; utter nonsense or gibberish should certainly be excluded from the definition of confabulation. In comparison with the themes of delirium, those of confabulation are stable and coherent as well as rational. Delay (10) obscured the definition of confabulation when he presented it as an example of delirium, as outside the realm of logical categories where the distinction between reality and imagination has been abolished.

Korsakoff (21) remarked on both labile and fixed delusions in his patients. While, on the one hand, confabulation should be distinguished from delirium and hallucination, on the other, it should not be confused with systematic delusions. These may indeed provide a dominant or secondary theme for the confabulation of the acute patient, and also carry over into the chronic phase. Delusions about the chronological setting of events, which can be fleeting or lasting are, of course, the very context of confabulation. They should be treated as a special class, and all other instances of a patient's methodical reconstruction of his perceptions and notions so as to fit a particular concern of his, had better be considered as delusional symptoms extraneous to confabulation.

Our review of the literature has posed a number of problems about confabulation. Some arbitrary decisions have been proposed in order to arrive at a convenient definition. According to this, confabulation is a factually incorrect verbal statement or narrative, exclusive of intentional falsification, fantastic fabrication, random guesses, intrinsic nonsense, the chaotic themes of delirium and hallucinations, and all systematic delusions other than those arising from the patient's disorientation in his experienced time. An attempt to justify this definition and to demonstrate its heuristic value will be made in the following section, by presenting our own observations and comments based on a study of sixteen patients in the acute phase of Wernicke's encephalopathy and twenty-eight chronic Korsakoff patients.

THE ACUTE PHASE

In the early stage of the Wernicke-Korsakoff syndrome, the patient is always severely confused and disoriented. He does not know, or only gradually realizes, that he is in a hospital. There is a gap in his memory for the period during which he was transferred from his domestic or work environment to the ward. Because of this lack of con-

tinuity, he seems unable to accept his new condition with the same conviction of reality as patients without amnesia register their hospitalization. If asked where he is, he may correctly state that it is a hospital or, perhaps, give its full name. This response, however, may be one he has learned rotewise, or may be determined by such cues as the interviewer's white coat and the row of beds in view. Even so, the patient's next answer to the same question, possibly only a minute later, is quite likely to refer to some different place. If he has been brought to the site of the interview from another wing of the hospital, he is almost certain to refer to either the former or the latter as some place other than the hospital. The difference is recognized but the transition is misjudged. The certitude with which the place is identified need not bear any relation to its accuracy. The notion that it is a hospital can be entertained very tentatively or fleetingly, the delusion that it is some rooming house or "The Eagle which used to be Mrs. Porter's tavern," can be held firmly day after day, or discarded at the first intimation of doubt.

The patient's errors in recognizing where he is apply also to his judgment of time and recognition of faces. His misjudgment of time is usually slight: a matter of days or weeks, at most a few months. Typically, the patient believes the time of the year is that which preceded his hospitalization. Visible evidence to the contrary is unlikely to change his judgment: though the trees may be in bloom outside his window, for him it is still December, just before Christmas.

Judgments about the time of the day are rarely better than a guess. Though the patient may give the year accurately, he is unlikely to be equally accurate about his age; that may be stated to be fifty at one moment, twenty-six at the next. He may arrive at the correct answer by subtracting from the current year that of his birth—a datum often available even to the severely confused patient. One of our patients who, knowing the current year, correctly calculated that he must be about sixty, soon afterward thought he was a young man and that we were in 1928. The interviewer corrected the error, stating that the current year was 1959. The patient's immediate reaction was an indignant rejection of this piece of information, but a few moments later he grudgingly accepted it, and to be quite certain asked the interviewer, "Did you say we are in 1959?" Then, in reply to the affirmative nod, he commented, "Oh, I thought it was more than that!" It would appear that, having first slipped back 31 years in his frame of reference, the patient quickly resumed that of the calendar year, but some memory—no doubt an irritating memory—that he had lost count of the years persisted.

Few patients in the acute phase learn the name of the doctor sufficiently well to reproduce it on demand, or even to decide with confidence whether the white coated man facing the bed is Dr. X or Dr. Y. A new name may be registered though so that it reemerges in a story about recent events, the substance of which, however, is drawn from an earlier setting. One patient, while admitting one moment that he had been in the ward for the past two weeks, in the next gave a lively account of his outing the previous Sunday, when he "remembered" having been taken to church in a car, and to dinner afterwards. The person who accompanied him in repeated versions of the story was alternately his brother and Dr. Y, one of the neurologists who had examined him. When asked whether he had seen the interviewer before, the patient's typical answer is that of tentative assent, hardly ever a firm denial. *Déjà vu* is a common symptom, and many authorities treat it as one aspect of confabulation. Furthermore, in the acute phase patients do not simply remark on the questioner's familiarity: they tend to identify him as a particular person, one whom they know well enough, though not by name—"the man who sold me the insurance," "the guy I often meet at the bar."

Whether misidentification of the environment is the substance of confabulation or not, it is the product of the same circumstances from which emerge the logically consistent stories that fully qualify for this description. The classic example is the patient's reply to questions relating to his activities earlier in the day or the previous day. He almost never answers that he had been lying in the same bed, been in the same spot where he is at the moment. Typically, he gives a coherent report of his activities at work, a day in the home, of a meeting with a friend, a visit to the doctor. A report of what the patient did at the shop in the morning, of whom he met the previous evening is confabulatory because it is given in answer to a time when we know he was laid up in the hospital. He does not know; that time does not exist phenomenally for him. There is nothing in his brain, at any rate nothing available for his cognitive function, which could provide the proper temporal context, and thus set off the correct answer or inhibit the incorrect one.

A convincing illustration of the importance of the temporal context was provided by a patient who had reached the end of the acute phase. She had five grown children spaced at intervals of two years from 32 to 40. When asked to state the ages of her children, the patient was as likely to begin with 40 and work her way down to 32, as to place them at equal intervals betwen 12 and 20. Once, after she correctly started with 40, 38, she was abruptly asked to give the age of the youngest. Her answer was 14, and she was not visibly disturbed when the gap of 26 years between the oldest and youngest was pointed out as an improbable one. She recognized the improbability of this situation, and obligingly raised the age of the youngest to 21. This kind of erratic shift in the chronological context was the source of practically all the confabulation on which Pick (28) founded his later theory. Because his patient would give his age as 19 or 20, then questioned about his children, would say he had four, and immediately state his age was 36, Pick concluded that the need to correct contradictions was deficient in Korsakoff patients. Any such need, however, presupposes an awareness of contradiction, and there is no reason for believing that this awareness was present in Pick's patient.

Our patient's concession in narrowing the gap between her oldest and youngest child from 26 to 19 years may be taken as an example of correcting a contradiction. It is even more definitely an example of the extreme lability any such data as ages or dates possess with amnesic patients. The information available to them is virtually unbounded by a temporal frame; it can be transposed with ease—and without any sense of contradiction—from one stage in the patient's career to another. Usually, it is transferred from an earlier period to the more recent past, which is not or barely is represented in the patient's memory. Hence Bonhöffer's dictum that confabulation occurs in order to fill gaps in memory or knowledge, a principle especially stressed by Bleuler (6). If the proposition is meant to be more than loosely descriptive, it suffers from ambiguity on several scores. It is misleading even as an analogy, because confabulation is not a process of interpolation, the fitting of some appropriate content between two or more firmly established landmarks; it is rather in the nature of extrapolation, when information on only one end of a series of events is available. As an explanatory formula, the gap-filling model is patently incorrect if it implies a deliberate substitution of some appropriate but incorrect information for the correct one. Korsakoff patients do not intentionally tell lies, neither do they prefer to supply any answer rather than admit ignorance when they know that the right answer is not available. If the gap-filling formula is meant to refer to an unconscious process of substitution, it explains nothing. In the first place it would be an untestable proposition; moreover the point of interest would then be why substitution

should occur rather than plain admission of ignorance.

Perhaps, the explanation is that the patient wants to oblige the interviewer, and that is why he invents specious answers. This motive may indeed operate, yet it would not result in confabulation but for a background of thorough confusion and incertitude. Nor is the desire to please a circumstance widely present in confabulation. The following example is drawn from an interview in which the patient was cantankerous and far from anxious to ingratiate himself with the questioner. He had but one child, a grown woman, mother of a young boy. When asked whether he had any children, he admitted to a young son. A few minutes later he was faced outright with a question about his daughter's age. He then answered: "My daughter? Well, one of them is 21 . . . The other one may be 20; one may be 52." Unless three is always better than one, this is hardly an answer to satisfy the interviewer.

Grünthal's (14) comment that amnesic patients confabulate because their thoughts are set on the wrong track, and they cannot get away from it, is nicely supported by this patient and by a woman in her thirties, Ruth, both of whom were interviewed at some length on three consecutive days. Even their boldest fancies did not get them off those tracks which can be determined by mood or affect as well as by theme or setting. The older man quoted above, for instance, responded the first day consistently in a querulous, suspicious manner. The second day he confided in the interviewer, but most of his information related to the ingratitude and the intrigues of various undefined people he was in contact with. The third day, the patient talked effusively and with tears in his eyes about the kindness of all manner of persons—"yes, everybody"—toward him.

In her first interview, Ruth drew her uncle into almost every answer she gave about her circumstances, and that included the hospital, for she was dimly aware of her surroundings. Next day her mother occupied the same central position; she had figured the first day too but in a lesser role, always referred to as if alive, though in fact she had been dead for some years. The husband was not mentioned until the third day, but then everything the patient talked about led to him. In the second interview, when asked about her husband, Ruth at first denied that she was married. Whereupon the interviewer pointed to her wedding ring, and asked why she wore one. Having convinced herself of its presence on her left ring finger, Ruth named in close succession three different husbands, none of whose names she bore. She also provided some background for these fictitious marriages, involving parental opposition and a few other plausible circumstances. Withal, she denied ever having lived with a man, or having lived outside her mother's house. Her story was confused, yet much of it could have been based on facts, on events dating back over ten years, the time before she had married.

Nothing in this situation or in the subsequent interview suggested that the patient had invented the three husbands in order to fill a gap in memory, let alone for the sake of pleasing the interviewer. She was too deeply absorbed in herself to take any but the most superficial notice of him, too strained to piece together an account of herself to spare any resources for the invention of fictitious information. A brief moment of insight into her condition produced no words, only a visible emotional shock, and tears of distress and bewilderment. Confabulation was resumed a minute or two later, by which time the cognitive as well as the affective registration of her disability had, by all appearances, vanished. On each of the three days, the patient structured her fragmentary but fluent talk around a different person—uncle, mother and husband in turn—and molded her physical surroundings so as to accommodate the visible sights and audible sounds of the ward as well as the characters

of her phenomenal environment. This endeavor one day placed the uncle in the hospital for blood transfusion, on the third it converted the hospital into the shop where her husband—her real husband—was working.

On the surface, all the attributes of creative imagination, instantaneous invention, and denial of memory gaps were present in these interviews with Ruth. Nevertheless, to account for her behavior in terms of those constructs would gratuitously involve cognitive functions which neither possess much explanatory value, nor strike the eyewitness as convincing in that setting. Ruth's fabrications could be taken to justify Bleuler's definition of confabulation as "free inventions taken as experiences"; but most other examples provided by amnesic patients would fail to fit the formula. They involve no invention, only gross transposition in context. Even the patient quoted, unaware as she was of her inability to give the right answer, did not confabulate to conceal her ignorance; her stories were not inventions mistaken for truth, they were involved and visibly desperate attempts to escape perplexity.

In contrast with Bleuler's definition, but in agreement with many other students of the problem, it is proposed that in the early phase of the Wernicke-Korsakoff syndrome: 1) Confabulation is typically, but not exclusively, an account, more or less coherent and internally consistent, concerning the patient. 2) This account is false in the context named and often false in details within its own context. 3) Its content is drawn fully or principally from the patient's recollection of his actual experiences, including his thoughts in the past. 4) Confabulation reconstructs this content, modifies and recombines its elements, employing the mechanisms of normal remembering. 5) This material is presented without awareness of its distortions or of its inappropriateness. 6) It serves no other purpose, is motivated in no other way than is factual information based on genuine data.

DERANGED TEMPORAL FRAME

Errors made in the temporal placement of events play a central part in the formulation proposed above. Other students of the Korsakoff syndrome have also noted this disturbance, and no one has attributed greater importance to it than van der Horst (17) who traced all the psychological defects of the amnesic patients to their failure to attach temporal signs to experiences. That theory postulates a primary sense for the flow of time against which events are registered. In contrast to van der Horst, most other students of the Korsakoff syndrome prefer the opposite view which regards awareness of time as a product rather than a condition of sequential experience. There are empirical data too which run counter to van der Horst's formula, or at least dispute its adequacy, though others seem to fit it very nicely.

Of all our patients, a woman in her sixties furnished the most convincing evidence in support of van der Horst. Questioned in 1959, she believed that the last war took place between 1914 and 1918, and distorted her not inconsiderable store of information for public events so as to fit into this temporal frame. She knew that Eisenhower was the President at the time, and that he had first made his name as "Commander-in-Chief of the army at the end of the last big war," during the presidency of Franklin D. Roosevelt. The patient also knew, though rather vaguely, about Hitler and Stalin, correctly recalled the events at Pearl Harbor, and was able to define the meaning of Prohibition and the Great Depression. When asked for dates, however, she placed those of Eisenhower's command, Roosevelt's presidency, and Hitler in the 1910s, and the Depression—rightly—before F.D.R.'s term of office. Here was a perfect example of events being recorded without their temporal marks, or of their being displaced from their

proper chronological setting. Perhaps, it is also an example of real creativity arising out of and possibly compensating for the amnesic disorder; for all these events, though squeezed into the short span between 1910 and 1920, retained their correct temporal sequence. To accomplish this feat required considerable skill; it also ran counter to van der Horst's thesis. Moreover, it was only the calendar of public events which came to an abrupt end about 1921; for her more personal experiences the patient's time scale extended all the way to the time of the interview.

The frames within which memories of events experienced and information otherwise acquired are set can be distorted in various fashions. The distortion of the temporal or sequential frame is not simple or unitary, and that is not the only frame bent out of shape in the amnesic syndrome. Nor is the collapse of the temporal frame unique to clinical psychopathology. The mystical leap beyond the constraints of time, and creative imagination which transcends it undoubtedly has something in common with the confabulation of the amnesic patient. No less certainly they also have something that is lacking in confabulation. Spontaneity, direction and internal coherence are plausible examples, but not invariably reliable criteria of distinction. It is as well to bear in mind that none of the mechanisms which operate in confabulation is exclusive to the pathology of cognitive function. It is parsimonious to hypothesize that some common psychological process operates whenever facts are ignored, and when the deviation from the mode established by the senses, reason, authority, consensus or common sense extends beyond the normal limits of error. Conceivably, this process interferes with an inhibitory function which normally restrains a spatially, temporally and experientially unbounded cognitive operation to the boundaries of verification and of the momentarily relevant.

Evidently several diverse agencies can effect such interference: artistic inspiration; ordinary wishes, both conscious and unconscious; the reversible physiological effects of pharmacological agents; and the more permanent destruction or loosening of the experiential frame of reference resulting from brain lesions. In the Korsakoff syndrome the continuity with the phenomenal self seems to be impaired; frames of reference no longer appropriate are insufficiently inhibited. The argument here proposed, although speculative in the sense that it cannot be tied to any circumscribed neuropsychological mechanism or system, is at least consistent with experimental findings (33, 34) concerning the inability of Korsakoff patients to inhibit manifestly inappropriate perceptual and problem solving sets. Furthermore, it also conforms with the conventional belief that recovery from illness is a change towards partial or full restoration of normal function, and toward an adjustment of the organism to its environment within its capacities. Thus, the Korsakoff patient, having been freed from the neurological symptoms of Wernicke's encephalopathy, does not lapse from a condition of lively creativity into a state of psychotic apathy, but rather takes cognizance of his mental impairment and learns to function within the limitations of his remaining cognitive capacities.

THE CHRONIC STATE

The transition from the acute to the chronic phase in the Wernicke-Korsakoff syndrome is marked by a remission of those peripheral symptoms which are associated with the first of these two names. The memory disturbance remains, and tends to gain prominence as the other symptoms gradually clear up. A certain measure of disorientation usually continues, and so does confabulation in some patients; but in others it virtually disappears. With the passage of time many patients gain more or less insight into their condition; they become aware of their disturbed memory function. Williams and Rupp

(38) suggested that confabulation stops as insight appears, and there is good reason for accepting their proposition. It should be added, though, that insight into their condition is by no means a general characteristic of the progress of the disease, nor even typical of the majority of the patients. Nevertheless, the tendency to confabulate diminishes, and may practically disappear in many patients who are not aware of their impaired memory function or of its severity.

Reports about the confabulatory behavior of chronic Korsakoff patients differ widely: many present this symptom as typical of the disease, others remark on its complete absence. The lack of unanimity in the literature is probably due to the small samples observed no less than to the ambiguity of the term. The majority of our 28 chronic Korsakoff patients furnished no evidence of confabulation, in the wider or narrower sense of the term; a few repeatedly discoursed in a manner which would qualify for that definition. In view of the considerable intra-individual consistency in this respect, it would be reasonable to attribute the persistence of confabulatory behavior to basic personality structure, as Williams and Rupp and others have suggested. Since, however, usually too little is known about the patient's pre-morbid behavior, the crucial evidence is lacking. Consequently one must entertain the possibility that confabulation in the chronic phase is a symptom of the same order and origin as the amnesic defect, even though contingent on and not proportionate to the severity of the latter. It may indeed be caused by another disease process or mental disturbance superimposed on and complicating the pattern of the amnesic syndrome. In spite of the ubiquity of confabulation in the early phase of the Wernicke-Korsakoff syndrome, this last hypothesis is not entirely gratuitous, in view of certain clearly paranoid or schizoid signs in some of the Korsakoff patients.

We have subjected our 28 chronic patients to systematic interviews, most of them repeatedly at intervals over a period of four to six years.[3] The interviews explored the patient's own past, his present activities, his notions about the research he participated in, his knowledge of some outstanding current events and of a few major crises in the past five decades, including the names of several famous or notorious political figures. Some of the typical or otherwise notable material obtained in the interviews has been presented in a previous publication (35).

While most of the patients correctly recognized the writer, two repeatedly expressed the mistaken belief of having met him in a different social setting prior to their hospitalization, and two or three patients regularly confused him with another doctor. A few of the patients on one occasion thought they had previously met his new assistant. More often, however, they expressed doubt when faced with an unfamiliar experimenter, and were more likely to deny having met one with whom they had in fact worked previously. Likewise, when we moved into our new laboratory, none of the patients questioned pretended that he had been there before. Errors about their own age or the ages of members of their families were quite common; sometimes relatives dead at the time of the interview were thought to be alive. Few of the patients had a clear or accurate notion about the circumstances of their original hospitalization; most of them reported it by mentioning some other medical or surgical complaint which may have been a complicating factor at that time or, more often, had occurred at a previous or later date. Typically they were well informed about any severe illness or surgery they had experienced during the chronic phase of their disease.

Negative replies were uncommonly frequent to questions about public events, even though dating back well beyond the beginning of the patient's amnesic disturbance.

[3] Since senility as a possible source of confabulation cannot be ignored, it should be noted that six of these patients were above the age of 65.

Much of the information available, while not incorrect, sounded just too terse, inadequate and stereotyped to be accepted as normal in the context of a leisurely interview, *e.g.* "Stalin—leader of Russia"; "Hitler—leader of the Germans"; "Churchill—leader of England." Undoubtedly, many of the patients had been unsophisticated and indifferent observers of world events long before their hospitalization, yet one would have expected them to say something more colorful about these three "leaders." They rarely did.

Displacement in time or condensation of two events, the characteristic feature of confabulation with autobiographical content, also occurred in the patients' recall of world affairs, especially arising out of a confusion of the two wars. At least one example of the opposite type of error, analogous to Pick's (26) description of reduplicative paramnesia, has been noted in this context when a patient, asked whether she remembered the names of any generals in the last war, answered: "Cushing,[4] Eisenhower. I don't mean the President; someone of the same name. But he spells it different." The same patient (A. B.), though quite reliable about conditions of her own life in hospital, described Stalin as a spokesman of Germany (another said he was "from England"), thought in 1960 that he as well as Mussolini were both alive, and gave the following answer when asked who was at the head of England: "I was going to say Churchill, but he isn't any more." So the interviewer helped, asking whether it was a king or a queen. "No," she commented, "hasn't England gone like America?" "You mean a republic," suggested the interviewer. "Yes, that's it. There is a queen."

Here a patient, who discussing her own life always talked sensibly, uttered in one breath two mutually exclusive statements, and thus furnished some evidence for Pick's (28) later formula of confabulation. Some of her remarks expressed that type of confusion which amounts to confabulation and her revision of the British constitution may even be treated as an example of fabrication. She had also failed to keep abreast of events, a very common omission of amnesic patients, and not unknown with persons who are not so afflicted. That she followed some changes in world events is shown by her knowledge of the name of the President at that time; some other patients believed we were still under the presidency of Coolidge or even Wilson.

There were three more types of unsatisfactory answers about world events which occurred with considerable frequency. One was this kind of meaningless statement: "The British fought on the side of the English." Another was evasive generalization, like the following comment made shortly after President Eisenhower's visits to a number of foreign capitals, which had received considerable publicity. "He travels quite a bit. He goes all over. Every time you pick up the newspaper you read Eisenhower is in this place or that place. I suppose he has to, it's his job." Last, and perhaps most characteristic of the Korsakoff patient, is the substitution of a true, but minor incident for the whole event. An example would be the naming of Poland as the only ally, or the history of the Second World War thus summarized: "The Germans fought the Italians, and the Americans went in." The patients who made these remarks had neither Poles, nor Italians nor Germans, in their immediate ancestry to offer a plausible explanation for their simplification of the event.

These examples, though varied and illustrative of how confabulation operates outside the autobiographical area of reference, nevertheless formed but a fraction of the answers. The patient's characteristic response was to give a brief reply—typically a single word—if the information was readily available, and beyond that plead ignorance. Only one of them (H.T.) made

[4] The name of the Cardinal Archbishop of Boston, presumably given instead of General Pershing.

any effort to maintain a facade of knowledge in spite of her evident amnesia for all relevant information. Her attempts at evasion were not particularly subtle, consisting of such phrases as "just events," "just happenings of the day," "I never remember wars. They don't mean a thing to me. Who cares about wars! I like to think about pleasant things. . . . The Depression—who wants to know about that! I like all the best places: the Ritz-Carlton. New York is the only place to live. I was there a month ago. . ."

Most of the other patients showed no reluctance to admit their ignorance, and doubted their knowledge even when it was accurate. Very likely, they were barely more conversant with world events before their hospitalization. Some of them readily admitted that they had a poor memory, or that they had known or read about the point raised, but no longer remembered what it was. Quite often the patients thought they had to explain their ignorance of current events: they never saw a newspaper, the television had broken down. Yet when asked, after two hours' experimental work, what they had been doing that morning, none of them took the trouble to find an excuse for recalling no more than "playing with cards" or "answering the questions you have asked me," which is all most of them could say. More patients must have been aware of their amnesic derangement than would admit it, yet even those who did admit it failed to realize its full extent. At times this awareness visibly disturbed a patient, but none attempted to conceal the defect by fabrication.

Their answers about personal matters no less than about world events tended to be brief, and were rarely expanded beyond a bare statement of fact. With a few patients, reminiscence about the past produced more fluency and spontaneity, as might an opportunity to air grievances or to complain about ailments and discomforts. Very probably, there was a high negative correlation between the rate of spontaneous verbal output and the veracity of its content, but usually the patients' faith in an inaccurate statement was easily shaken. In their diffidence, their readiness to admit ignorance, patients in the chronic stage notably differed from those in the acute phase; neither did they talk any gibberish.

In order to arrive at a crudely quantitative survey of the confabulatory content, the autobiographical information given by 28 patients was analyzed on the following criteria.

1) Does the patient reliably know that he is in a mental hospital, that he has been there for some months or years, and does he relate his reports about himself to that setting?

2) Does the patient's account of his past tally with the known facts, or does some of it conflict with the records, with his own statements, or with common-sense?

3) Is the patient's account of his past and present distorted by some delusional theme?

All but four of the patients interviewed seemed to be clearly aware of their circumstances in repeated interviews. Two (E.F. and L.H.) may not have had any stable, clear notion about their condition; one (H.T.) probably did but refused to admit it; a fourth (N.M.) understood the situation somewhat dimly, and was prone to regard her state as one of temporary hospitalization, ostensibly not for any mental derangement. It is significant that these four patients were among the oldest of our sample, all in their seventies or eighties. Twenty-three patients always discussed their situation within the realistic context of the mental hospital. The twenty-fourth (H.S.) knew his situation perfectly well, but in the course of his animated talk was liable to be carried away and speak as if his presence in the hospital was of a purely transitional character. He was, however, easily brought down to earth, and more often than not reminded himself of the actual state of affairs.

Two patients (L.H. and H.S.) quite certainly presented their past in a more favorable light than any truthful account

could have done. Three others (A.B., H.T. and S.M.) probably embellished on the bare facts of the past. This short list, of course, does not include the patients' reconstruction of the circumstances which led to their hospitalization. Hardly one of them attributed this to a breakdown brought on by prolonged alcoholism, though several of them readily admitted that they used to drink heavily and to neglect their diet at the time.

In contrast, Ruth would blame alcohol for her tuberculosis which in fact had laid her up for many months. This patient, whose free-flowing confabulations were described in a previous section, was interviewed again two and three years later, during which she had undergone the change characteristic of the progress in the Wernicke-Korsakoff syndrome. Now she described the conditions of her home and work prior to hospitalization quite accurately and in fair detail, though she could not say much about life in the hospital. She was somewhat apathetic, rather taciturn, diffident, and easily given to tears at the realization of her deteriorated state. She did not confabulate any more, but frequently and without apparent reason in the context of the interview or testing session, declaimed: "I am an American citizen, fighting for freedom." Once she added: "I want to stand on my own feet, to be a housewife," which may explain her stereotyped phrase as a claim to be discharged from the mental hospital. Yet she did not protest overtly against her hospitalization, expressed profuse gratitude for having been cured of t.b., and would "Thank God" for as small an achievement as answering a simple question without error. She is listed among the eighteen chronic patients who showed no sign of confabulation according to our criteria. The other ten, including three who produced delusional material, will be briefly discussed below.

Undoubtedly H.T. presented the most puzzling problems of confabulation. She was a woman in her late sixties or seventies who denied all the unpleasant circumstances of her life with impressive consistency. After ten years' hospitalization she still maintained that she had been brought in the previous day for observation, and would be sent back within a day to the hotel where she lived with her husband, and conducted the life of a well-to-do lady of leisure. Her stereotyped account of her past life was probably based on actual experiences, but she seemed unaware that the conditions of that very agreeable life had long since ceased to operate. It was often difficult to believe that H.T. was genuinely so completely unaware of her present circumstances as she appeared to be, so majestically indifferent to the contradictions and sheer nonsense of the remarks with which she tried to parry the unwelcome effects of her amnesia. When asked to name the title, the author or the contents of a book she had been reading, she would brush aside the question with some comment as, "I am too busy to care about those things," or "I am just not interested in that." The food she reported eating was always excellent, and often included dishes which are not the staple diet of hospital meals. The only reason she drank nothing but tea with those meals was her indifference to any alcoholic beverage other than champagne, and that clearly was not served in the cafeteria. While so careful to maintain the illusion of her privileged social position, H.T. never once spoke disparagingly or resentfully about the setting of the mental hospital or her fellow inmates. All that barely existed for her. One would think that an elaborate facade like hers could not be sustained without immense effort, yet not once did H.T. betray her disbelief in anything she pretended. She was the one Korsakoff patient responsive to hypnosis, but repeated attempts with this technique resulted in absolutely no new material of recall, in no admission of her awareness of her condition. Her case allows for various interpretations, but fundamentally

it fits the formula of confabulation arising from an arrest of experiential time. Motivational influences undoubtedly played their part in producing her glib evasive answers as well as in her obstinate adherence to a past image of herself, but the mechanism by which such influences interacted with the amnesic derangement could not be determined. H.T. was the only one of our chronic patients who could not name the year of her birth, and the indignant tone in which she professed her ignorance suggested that the information was not beyond the reach of recovery.

E.F., another woman in her seventies, remembered in considerable detail the circumstances of her life in the parental home and the family business, but apparently nothing about the several decades which had since elapsed, and that included her marriage to a man who had been dead some twenty years. So little did she take cognizance of her own, wizened, bent body that on one occasion, asked what she would like to do when she left the hospital, E.F. answered she would like to go out to dances on Saturday nights. N.M. remembered very little about her past, except the dates of a few early landmarks, and an automobile accident. The latter she could not place in time, but believed that her husband was killed in it, and that she herself was in the state hospital for the injuries sustained on that occasion. In fact her husband died only a couple of years prior to the interview, in ripe old age and long after the patient's admission to the hospital. Her oldest child was in her mid-fifties; she thought of her as aged 24, and the discrepancy between these two ages about measures the gap between the actual and N.M.'s phenomenal world.

L.H. was in his eighties when we last interviewed him. He firmly believed that he had just driven over in a wagon from the family farm where he was working. He stated his age correctly, yet thought both his parents were still alive and dependent on his support. He seemed completely unaware of his surroundings, did not appear to know or recognize Boston, and had forgotten all about the last few years prior to his hospitalization at the age of 78. This man's confabulation, however, extended beyond errors attributable to the gap in a life phase remembered. Asked whether he had ever been in the army, he answered that he had served in Canada during the World War—presumably the First. In reply to further questions, he affirmed that he had seen battle and had fought the enemy: the Germans. Had he fought the Germans in Canada? Indeed he had, they had gone over there, though not in full force. Having mentioned the war, the patient then readily blamed it for his confusion about other matters concerning his activities.

H.S. was interviewed many times during the six years following his admission at the age of 63. He always knew his age, and spoke of it with as much pride as about any other achievement for which, rightly or wrongly, he took credit. He had little tolerance for any insult to his self-esteem, and accepted his hospitalization only as a temporary measure to help him over the crisis which his wife's death had brought upon him. He admitted to a breakdown the more readily as he felt certain he had not gone crazy. He was clearly unaware that his wife had been dead for as long as sixteen years prior to his first hospitalization, and seemed to have no recollection of living as a widower with another woman, a fact entered on his case record. Often he would think that his home, managed by a sister-in-law, and his two daughters were there waiting for his return that very night. These women, well in their thirties, had long since ceased to take any interest in their father, yet H.S. would boast of their affection and of the fidelity of their visits. First, he always talked of them as little girls in their teens, then reminded of his own age he would realize that they too must be older. As for their being married and having children, he was vague and always admitted ignorance rather than fabri-

cate information. Reminiscences about his own past, on the other hand, tended to be more colorful and flattering to his self-regard than the dry factual accounts typical of the other Korsakoff patients. It is unlikely that, as a salesman and then working in a warehouse, he had been quite so splendidly successful as he would have us believe, or that having avoided the draft—"I was given a lucky number"—he had nevertheless made "the best soldier you ever saw."

The autobiographical reports of two other patients, A.B. and S.M., were just too rich in otherwise quite normal exploits to fit into the life of even a very enterprising middle aged woman, especially if she had been repeatedly hospitalized for shorter or longer periods on account of her alcoholism. A.B., who in fact has been a nurse, on one occasion described a very creditable war record, of which her file makes no mention, but never once talked about her first illegitimate child or a second and third marriage which are data listed there. S.M. seemed at times to have a hard job in sorting out fact from obfuscating accretions to memories of the past. Perhaps her goal was to maintain a satisfactory self-image, adventurous yet respectable. The effort never resulted in a very clear picture; the incidents sounded probable enough though too crammed in time. Distortions in the temporal frame of reference were more evident here, though barely drastic enough to amount to confabulation.

Of the three patients who furnished delusional content, no more will be said about H.S.; his delusions rarely intruded into his grasp of the world around him. A.I., a woman studied in her late fifties, had two widely disparate delusional themes, neither of which figured prominently in her discourse. The one concerned her husband's schemes to keep her in the hospital, so that he could live with another woman and spend unimpeded the sum he had drawn from her account. She always described in the same details and in much the same phrases an occasion when he had unjustly accused her of infidelity, and a later incident when he had her arrested on a bench in the park. According to her file, there seems to be little substance to these grievances. The patient was indeed arrested, in fact on three different occasions, for drunkenness. She herself had withdrawn her bank balance, and her husband was in a sanatorium at the time of her hospitalization. The record also mentioned that A.I. was spreading untrue rumors about fellow patients in the hospital work room. Her other delusional theme was that a fellow-patient had stabbed her leg with a sharp pencil without provocation. This was her story in the third year of hospitilization when she had scalded her leg, and again three years later when she injured her other leg in a fall. On that occasion she vaguely remembered the first accident, and correctly stated that it happened to her other leg. The foundations to these imaginary assaults were presumably two attacks she had sustained from fellow patients, neither of which had injured her leg.

M.O., a woman in her fifties, alternated in her interminable talk almost exclusively between involved conspiracies against her financial interests, and confidential complaints about her health and its malicious neglect at the hands of the medical profession. She differed from the other two patients in that the delusional content virtually monopolized her verbal communications, but like A.I. she was always clearly aware of her surroundings. Her memory for some incidents which could be woven into her delusional structure of the world was uncommonly exact. M.O. was an entirely atypical patient in our group; the Korsakoff syndrome, complex though it be, nevertheless formed only part of the total pattern of her psychosis.

If we dismiss the delusional contribution to confabulation, all the examples considered clearly originated in an effort to supply the information requested. In the process of

doing so, some confusion might indeed arise or the need to flatter one's self-image might assert itself. Typically, confabulation stems from indirection, from a precarious anchorage in the actual present. The effect of the amnesic defect in Korsakoff's syndrome is that a long sequence of experiences and observations is missing from the patient's awareness; they were never sufficiently structured to provide a frame of direction. Thus his momentary orientation may contain little or virtually nothing of his actual setting in the present, or combines elements of this with others drawn from his past. The resulting confusion may be resolved by an admission of ignorance or uncertainty; it may also lead to a substitution of memories from a long bygone time for those of the present and the immediate past. Naturally enough, any inclination to distort factual data, which may originate in the patient's peculiar personality or some mental disturbance independent of the amnesic syndrome, will be augmented in effect through the disruption of a realistic frame of direction and in the absence of discordant information.

EXPERIMENTAL DATA

Our experimental program included several tests which afforded ample opportunity for the application of an inventive genius or for the propensity to fabricate. An earlier paper (36) reported in some detail the reproductions of short narrative passages given by Korsakoff patients after very brief, and longer intervals. The universal trend they showed was one of levelling, of drastic impoverishment in content. In the acute phase loss of content, frequently amounting to one hundred per cent, was the only result observed. With chronic patients, elaboration, distortion of details, substitution and importation of material from outside also occurred. The modification of the original text by these means tended to be the same as occurs under conditions of experimentally induced ambiguity and delay with normal subjects. If we allow for the fact that Korsakoff patients register the original information less accurately and then forget more rapidly, their alteration of the content in recall was neither more arbitrary nor more profuse than in normal remembering. The most remarkable examples of importation and substitution were those which recurred in response to identical cues in much the same thematic form after a considerable lapse of time, such as a fairly detailed story about a farmer in place of a story about a cowboy. A stable fault in coding seemed to be the most suggestive explanation of this type of error which, as a matter of fact, has much in common with such delusional notions as those expressed by A.I.

Another probable source of confabulation was brought into light by a patient's reproduction of a short report concerning a shipwreck, which quite gratuitously stated that "all the passengers were tossed into the sea." According to the original text, "the passengers . . . were all rescued, though the boats were tossed about like corks in the heavy sea." Evidently, the patient retained the word "toss" outside its context, and built a new phrase around it. This may indeed serve as a paradigm for the confabulatory mechanism: a fragmentary, isolated memory becomes the anchoring for recall, and being but a fragment it is as likely to evoke an incorrect context as the correct one.

In several other experimental procedures, Korsakoff patients were as ready to admit failure, ignorance, inability to solve the problem, as were control subjects. An example is furnished by their records on an auditory test in which single words were presented aurally at the threshold of intelligibility (37). Failure of correct reproduction could take the shape of an incorrect response or no response. In a group of 21 Korsakoff patients the mean ratio of no reply to incorrect response was 1.41, in a control sample of 18 it was 1.13. Apparently the former were not more inclined to please the experimenter with any likely response,

and not less liable to admit failure. A similar observation was made in an experiment testing the immediate and slightly delayed recall of lists of syllables (32). Although the material—particularly the nonsense lists—being poorly structured was eminently suited to distortion or fabrication, the Korsakoff patients made more errors by omission than by the substitution of incorrect syllables. This is hardly the result one would expect if it were true that Korsakoff patients confabulate because, with a complete disregard of facts, they will draw on any source to fill in gaps of memory.

SUGGESTIBILITY

Pick (27) and Körner (21) were among those authors who attributed the confabulation of Korsakoff patients, in part at least, to their unusual suggestibility. Their observation was true enough if it implied that most amnesic patients would not display confabulatory behavior unless insinuated, or pushed into it by the interviewer. It is true also in the sense that suggestibility is indirection, a lack of anchoring, *i.e.*, the same condition which is manifested in perplexed or confused behavior. Korsakoff patients would therefore be marked by Eysenck's (11) secondary suggestibility, *i.e.*, gullibility, but need not necessarily be characterized also by primary or ideo-motor suggestibility. As a matter of fact, with one exception, our chronic Korsakoff patients proved absolutely resistant to hypnosis—with or without pharmacological aid—and thus, according to Furneaux's (13) criterion, they would score low on primary suggestibility.

There are good reasons for arguing that even their gullibility is manifested only in situations of deep perplexity. In one experiment a group of Korsakoff patients and a group of matched control subjects were individually presented with a selection of ten small bottles, each containing a different aromatic oil and appropriately labelled. Two of the bottles, however, were dummies, containing tap water. In predetermined sequence, the bottles were slowly moved inch by inch along a platform at the subject's neck level. The ostensible purpose of the experiment was to test olfactory acuity, and the subject's instruction was to signal as soon as he could smell the scent. The experimenter then noted down the position on the scale, cleared the air with an electric fan, and brought on the next bottle. Positive response to the dummies was no more frequent in the Korsakoff than in the control group. Neither did one of the twenty-one chronic patients tested fall for the trick in which the interviewer asks him jovially, "Now didn't we meet over a drink last night?" Perhaps this interviewer's voice just did not carry enough conviction, for every one of the patients answered negatively, some smiling at the joke, others more earnestly pointing out that last night, as every other night, they had been in the hospital where no drinks are served.

Admittedly, in the early phase of the Wernicke-Korsakoff syndrome the patients are highly suggestible, but that state changes as their confusion clears up, or persists only to the extent that their cognitive framework remains diffuse. One of the most stable defects of the chronic amnesic patient is his inability to record the recurrence of the same or closely similar events. This is also the dilemma on which he is most susceptible to non-veridical suggestion. He is likely to consent without demur to the suggestion that he had seen a person only once before, even though he had done so many times, or that he had been at a place frequently, while in fact this may be but his second visit there. Similarly, a patient who, like L.H., is prone to slip from the present into the past frame of personal reference, barely needs a leading question to furnish his surroundings beyond the walls of the room with objects and people from the past. Suggestibility like confabulation in Korsakoff patients is coextensive with the degree and the reference area of their confusion.

SUMMARY

An attempt has been made to define confabulation. Brief verbal statements and more extended accounts, if factually incorrect, were alike admitted within the definition proposed, but the following were excluded: intentional deception, fantastic fabrication, wild guesses, gibberish, the productions of delirium, and delusions which distort the image of the actual world. The content area of confabulation is typically, but not exclusively, autobiographical; its source is predominantly that of the patient's actual experiences in an earlier phase of his life.

In the acute state of the Wernicke-Korsakoff syndrome, confabulation forms a considerable part of the patient's verbal communication, and some trace of it can probably be recognized in every patient. It arises from the more or less total disruption of the patient's temporal frame of reference, and its content is typically an accurate statement displaced in its temporal setting, *i.e.*, a memory of the earlier past presented as an event in the present or the immediate past. Because of their extremely labile temporal anchorage, memories drawn from different chronological periods tend to become confused. Confusion between memories of actual personal experiences and second-hand information or mere ideas also occurs. A dominant theme or affect of the hour may violently distort the content of confabulation into a forcibly uniform pattern.

Such temporal signs as are attached to memories in normal cognitive function are indeed largely destroyed in the amnesic syndrome. This loss, however, is more probably an effect than a cause of the amnesic disturbance. Contrary to van der Horst's view, Korsakoff patients are not completely unable to reconstruct memories in their correct temporal sequence.

In the chronic phase of the disease confabulation is much less common, and may not be detected at all in many patients' discourse. This symptom undoubtedly recedes with the acquisition of insight into the amnesic derangement, but the latter is not a necessary condition of the disappearance of confabulation. The majority of the 28 chronic patients in the study reported always reliably knew their circumstances, and realistically discussed the events of their lives within the setting of the mental hospital. Only a few talked as if they were living in the remote past. Their answers to questions relating to world events or their personal lives were factual, usually correct or plausible, though some characteristic errors and distortions did occur. Many of the latter could be attributed to a desire to maintain an unrealistic self-image. This drive was very strong in one, and marked in two or three other patients, but apparently absent in the rest. Most of them readily admitted ignorance, and often spoke with undue diffidence. Not one example of truly fantastic fabrication was observed in the chronic patients, and delusional content either played a very minor part in their discourse, or could not be attributed to the Korsakoff syndrome. A marked degree of intra-individual consistency in confabulatory behavior or its total absence, supports the view that this symptom is an interaction effect of the amnesic syndrome and basic personality structure.

Suggestibility has not been found a useful explanatory concept. Korsakoff patients resist hypnosis and are not particularly gullible, except in areas of perplexity.

Confabulation, as a symptom in the Korsakoff syndrome, can properly be regarded as secondary to the amnesic derangement. It occurs within contexts which, for the lack of adequately structured memories, are diffuse, devoid of points of anchorage and orientation. The patient himself is unlikely to be aware of gaps in his knowledge when he confabulates; he transposes information from an earlier phase of his life, condenses or distorts material without consciousness of his deficit or of the confabulatory process, and hence without intent. Even in this situa-

ion of indirection and confusion, confabulation need not arise, or may be elicited only by leading questions. The amnesic deficit creates an occasion for its occurrence, and dispositions characteristic of the individual patient will determine its presence, rate and quality.

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PSYCHOLOGICAL STRESS: A RE-EVALUATION

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INTRODUCTION

The objectives of this paper are to re-evaluate the conventional concepts of psychological stress and to submit a concept which relates stress to the emotion of depression as well as the emotions of anxiety and anger.

Stress is a word that has had varied and changing meanings. Originally it was probably derived from "distress," and in common usage has come to indicate hardship, adversity, and sometimes forces or pressures applied to a person to compel or extort. In physics, stress denotes internal forces which resist alteration of form, size, or other material change which is known as strain. In biology and medicine, stress is a relatively recent term used to indicate either external forces and stimuli or their effects within the organism.

Stuart Wolf (76) regards stress as an external stimulus or force which is strain-producing to the organism. Harold Wolff (77), following the usage of physics, defines stress as internal resisting forces which the organism activates in dealing with noxious external stimuli. Selye (64) defines stress as a specific complex of bodily reactions induced by external forces designated as stressors. Rees (58) defines stress as any stimulus or change in the external or internal environment which disturbs homeostasis and which may result in bodily disfunction and illness. If this stress can be shown to have psychogenic as well as physiogenic factors, the resultant illness is considered to be psychosomatic.

STRESS MODELS

Historically, the first psychosomatic or psychological stress model was formulated by Freud (23) in his papers outlining the dynamics of conversion. Freud, in his own writings, restricted the application of the conversion model to hysteria *per se*, and in his paper on psychogenic disturbances of vision (26) he indicated that structural pathology of an organ may be the result of physiological disturbances which arise from the organ's abnormal use for the discharge of affects. Ferenczi (20) was the first psychoanalytic writer to apply the conversion model to a case of organ neurosis. Melanie Klein, Garma and Felix Deutsch (14, 28, 41) have further extended the conversion model to apply to all cases of organ neuroses. The conversion model postulates that somatic symptoms or disturbances have a meaningful symbolic relationship to the psychic stress; that is, there is a symbolic use of the body or bodily parts to defend against the experience of anxiety. Stress produces anxiety, which is converted or somatized into pathophysiological processes and structural change. The exact physiological mechanisms involved in the magical leap from the psyche to the soma are not spelled out in these theories. The conversion model may be stated schematically as follows:

Stress → Anxiety → Symbolic bodily activity

↓

Tissue changes which may have symbolic meaning

Flanders Dunbar (15) made an early attempt to integrate Freud's psychology with Cannon's emergency theory. She rejects Freud's model of conversion for the vegetative neuroses because she could not establish a symbolic choice of organ in most of the psychosomatic illnesses. She points out that Freud postulated an economic factor as well as dynamic factors; that is, when emotion i

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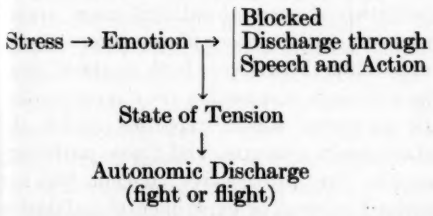
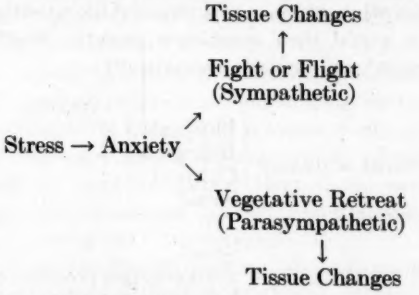
blocked from normal discharge it does not cease to operate but finds other channels of discharge. Energy denied access to normal channels of speech or action seeks discharge through the autonomic nervous system. The patterns of discharge are correlated with specific personality types. If the psychic apparatus is unable to discharge energy a state of tension builds up with the individual poised for either fight or flight. Dammed up energy spills over into autonomic and humoral pathways resulting in physiological disturbances and psychosomatic illness which are characteristic of the individual's personality type. Stated schematically:

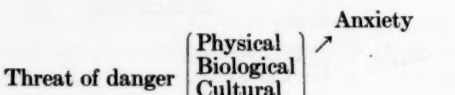


Alexander (1) also rejects the conversion model of vegetative neuroses because he feels that bodily symbolism can only be expressed by the voluntary sensori-motor system. For him, psychosomatic disturbances are vegetative responses associated with specific chronic nuclear emotional conflicts. Discharge of emotions denied adequate expression is thus shunted to the autonomic nervous system. The pattern of discharge is specific for the configuration of emotional conflicts and is intensified in degree and prolonged in duration. This excessive autonomic activity leads to disturbances of function which often end in tissue pathology. Stress is an anxiety-producing situation which is defended against by two basic emotional attitudes: 1) preparation for fight or flight; and 2) withdrawal from outwardly directed activity (vegetative retreat). The first attitude is correlated with sympathetic activity and the second with parasympathetic activity. Anxiety sets off a psychological chain of events which determines the physiological responses. Anxiety, the first link in the chain, is not a specific factor. Specificity is determined by the way in which the individual deals with anxiety. Stated schematically:



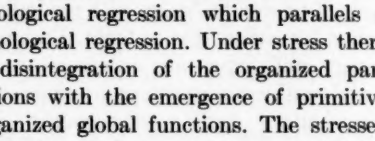
Wolff (77) sees psychosomatic diseases as the inappropriate use of protective adaptive responses to adverse or stressful life situations. This is an extension of the thesis of Claude Bernard that disease is the outcome of attempts at adaptations to noxious forces. Stresses that affect man arise not only from physical and biological dangers but also symbols of past dangers, failures, frustrations of needs and aspirations, cultural pressures and rapid social changes. An individual's responses to these stimuli are made up of conscious emotional attitudes and feeling tones, plus non-specific organ reactions which are always the same in all organs—swelling, hyperemia, hypersecretion, hypermotility. The disease patterns are not specific for noxious agents but are defense reactions of a non-specific nature. An individual may react to many different kinds of noxious agents in a similar way; that is, he may be predominantly a "nose reactor" or a "colon reactor" regardless of the nature of the stimulus. The particular way an individual reacts is similar to the way his stock (family) reacts; that is, the choice of organ is "stock bound." Constitutional factors, conditioning and cultural patterns seem to determine the dominant protective adaptive pattern for the organs involved. There is a type of specificity in the sense that individuals who react with changes in the same organ must also experience the same emotional attitude and feeling tones. Although

Wolff does not define stress in psychodynamic terms, it seems a fair inference that if an individual did not develop a protective adaptive response to a stressful life situation he would then experience anxiety. Wolff's model represented schematically:

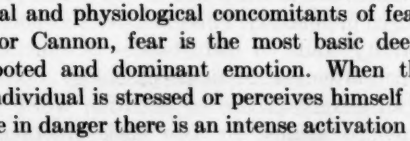


Ruesch (62) attempts to explain psychosomatic illness as the result of a breakdown in the ability of individuals to communicate. Patients with psychosomatic diseases are viewed as having a constitutional failure of development of the higher neural centers concerned with symbolic processes. Therefore, their mastery of the outside world breaks down because of their inability to manipulate symbolic processes for the purposes of communication and they attempt (regressively) to use shorter routes of communication, which lie partially within the body, to discharge emotional tension or anxiety. Energy that was used in effective communication now is redistributed through smaller arcs of inadequate communication which involve bodily processes. This use of shorter, more archaic channels for discharge of tension, in lieu of effective communication, results in a condition in which there is chronic vegetative autonomic hyperactivity and pathological changes. Ruesch's theory takes inadequate account of endopsychic and psychophysiological processes. It is essentially a social theory.

Margolin (51) and Grinker (34) postulate a concept of physiological regression within a broader conceptual model adapted from the field theory of physics and experimental morphology. They view the neonate and his environment as a relatively undifferentiated biological field without clearly defined boundaries and with whole patterns of response. Maturation is accomplished by differentiation of part functions which are then integrated into new intra-personal transacting systems. They theorize that when an individual is stressed there is a physiological regression which parallels a psychological regression. Under stress there is a disintegration of the organized part functions with the emergence of primitive unorganized global functions. The stressed individual not only resorts to more primitive psychological mechanisms but there is a reactivation of visceral patterns more appropriate to earlier phases of development. Because adult tissues have been rendered more rigid through maturation they do not tolerate regressive whole responses, which the infant easily tolerates, and tissue pathology results. This provocative concept has not received clinical or experimental validation and has been very seriously questioned by Mirsky (55) and Mendelson *et al.* (53). Stated schematically:



Cannon (9) viewed psychological stress as anything which would excite the psychological and physiological concomitants of fear. For Cannon, fear is the most basic deep-rooted and dominant emotion. When the individual is stressed or perceives himself to be in danger there is an intense activation of the sympathetico-adrenal system which, if continued, results in tissue pathology.



Seyle's stress model (64) is essentially an

extension of Cannon's concepts. Cannon's work focuses on adrenal medullary and sympathetic activity. Selye's work includes the entire adrenal with especial emphasis on adrenal cortical functions. These adrenal cortical manifestations may be viewed as second-line defenses mobilized to support the more easily aroused but evanescent sympathetico-adrenal activity. Adrenal cortical activation produces manifestations of the general adaptation syndrome (GAS), which includes hypertrophy of the adrenal cortex, shrinkage of the lymphatic organs, gastrointestinal ulceration, and alteration of the chemical composition of the body, etc. Selye's stress concept has been built on the basis of work with agents which are unconditional disease producers, such as extremes of temperature and severe mechanical injuries which produce a direct effect on the body. Selye's model has received inadequate validation as an appropriate mode of reaction to psycho-social stressors. Its schematic representations:

Stress → General Adaptation Syndrome

↘

Tissue Pathology

STRESS AND ANXIETY

In all of the stress models discussed above, psychosomatic illness is considered as a variant or the equivalent of neurotic illness. Anxiety is the organizing principle around which the psychodynamics of these conditions are formulated. Psychological conflict or stress produces anxiety, which is an experience so painful to the individual that he defends against it with the symptomatology of neuroses, psychoses or psychosomatic disorders. This concept of emotional disorders depends on a unitary hypothesis of emotion first formulated by Freud (24), and most recently restated by Blau (6). This hypothesis holds that anxiety is the basic or primary emotion of displeasure and the source of all other emotions of displeasure. The infant at first is only capable of the experience of anxiety. Maturation results in a differentiation of other displeasurable emotions such as rage, depression, guilt, shame and disgust. One of the reasons that Freud designated anxiety as the key affect is that he considered it to be the only displeasurable affect which is always accompanied by motor discharge. He stated that if motor discharge accompanied other displeasurable affects such as grief or sorrow then these were secondary reactions but not essential constituents of the total response (24).

When the models discussed above focus on psycho-physiological mechanisms involved in psychosomatic disorders reference is usually made to Cannon's fight or flight principle with sympathetico-adrenal activation. Stress is represented as an excitatory phenomenon. The only exception is Alexander's concept of vegetative retreat, which is an inhibitory-conservatory phenomenon mediated by the parasympathetic system. This presents the paradox of a basic excitatory state (anxiety) finding expression through inhibitory-conservatory channels (vegetative retreat). If anxiety is basic, then there should be evidence for this in terms of sympathetic activity.

To accept Alexander's concept of vegetative retreat it is necessary to assume that the physiological manifestations of the underlying state of anxiety are not expressed but are negated or masked by the physiological accompaniments of counterphobic attitudes which the patient mobilizes in an attempt to defend against the basic anxiety. There is little doubt that masking of one affect by another occurs at a psychological level but there is little to support the postulate that there is a correspondence at a physiological level. It hardly seems reasonable that a counterphobic reaction would be the primary source of a psycho-physiological response, although it could be expected to modify an ongoing process. A simpler and more economical explanation is possible if one accepts the hypothesis that stress may directly evoke inhibitory-conservatory responses as

well as excitatory. Biological and psychological evidence supports this point of view.

THE PHENOMENON OF SUDDEN DEATH

Cannon (9), after an intensive review of the world literature and communications with responsible individuals who had made first-hand observations of "voodoo" death, concluded that it must be a real phenomenon explainable in terms of natural processes. He postulated that such deaths are due to physiological imbalances resulting from an intense excitatory state with excessive sympathetico-adrenal discharge. However, there is an interesting paper by Richter which permits an alternative explanation. Richter (61) found that wild rats put into a container of water from which escape was not possible died in a few minutes. This was in marked contrast to white laboratory rats which swam for 40 to 60 hours before drowning. Cardiograms obtained by attaching electrodes to the animals before they were put in the water showed an increase in heart rate in the white laboratory rats; the wild rats showed a cardiac arrest after an initial but short increase in rate. Autopsies revealed the hearts of the wild rats to be in a state of diastole and filled with blood, whereas one would expect a heart contracted in systole if death were due to the sympathetico-adrenal mechanism of Cannon. If the wild rats were handled and freed repeatedly before they were placed in the experimental containers, they behaved like the white rats and attempted to escape by swimming for the same length of time. Adrenalectomized wild rats were not protected from sudden death and cholinergic drugs made white rats prone to sudden death. A few wild rats died simply while being held in the hand. Richter points out that this hardly is a fight or flight response. Rather it seems to be a reaction of hopelessness and despair. The animals simply gave up.

Richter suggests that the "voodoo" death phenomenon in man is similar. The "boned" victim, like the wild rat, views his situation without hope and resigns himself to his fate. He is not set for fight or flight and his death is not sympathetico-adrenal as Cannon postulated. Richter's mechanisms also seem to allow a more adequate explanation of psychogenic deaths reported in inmates of concentration and P.O.W. camps. These individuals, overwhelmed with despair and hopelessness, took to bed, turned their faces to the wall and died. Physician inmates (57) of such camps reported that these deaths could be averted if the individuals could be stimulated into anger or some interest in events around them.

STARVATION

Hunger is a warning mechanism which signals long before there is an actual tissue depletion. Hunger often produces a sense of tenseness or anxiety. Carlson (10) found that during hunger contractions the nervous system is in a state of greater excitability as reflected by increased tendon reflexes. Wade (73) made similar observations regarding muscular and mental performance. However, in starvation where there is actual tissue deprivation this excitatory state gives way to a profound inhibitory-conservatory state. Keys (38) reports that the starving man has a low metabolic rate, bradycardia, hypotension, hypothermia and great muscular weakness. Although there can be no argument that starvation is severe stress, it is of interest that in man neither acute nor chronic starvation produces an increase in 17-ketosteroid excretion (39).

The starved man states he is depressed and his facies suggests depression more than emaciation. He becomes increasingly quiet, somber, apathetic and slow in motion. Although he reports increasing irritability there is little overt indication of it. The starving man conserves energy in every possible way. Neurotic symptoms are the rule although psychotic symptoms may appear. In the Minnesota Experiment the

MMPI mean profile of 36 "normal" young men showed normal T scores on all scales before starvation. Average starvation T scores for depression and hypochondriasis were above 70, more than two standard deviations beyond the normal mean. Almost without exception the greatest change was in the depression score. There was a full return to normal on nutritional rehabilitation.

Hibernation and estivation (3, 37) are dormescent states of which certain animals are capable in order to adapt to seasonal diminution of food supplies or other stressful environmental conditions. Frank (21) reports that even man has a capacity for what might be called winter sleep in Arctic regions. Russian peasants when confronted with scarcity of food in winter have been reported to have existed for weeks in a somnolent state, arousing only occasionally for a scanty meal.

SLEEP DEPRIVATION

There are a number of studies which indicate that sleep deprivation is psychologically stressful (7, 8, 43, 72). It universally produces increasing fatigue, psychomotor retardation, irritability and recurrent waves of inhibition and depression. Prolonged sleep deprivation is ego disruptive and results in psychotic-like symptoms such as illusions, hallucinations, marked mood alterations and personality disorganization. In a recent study (52), polygraphic recordings of autonomic functions showed a definite parasympathetic shift as indicated by increases in skin resistance, decrease in both systolic and diastolic blood pressure, drop in body temperature and decreased heart rate. Excretion of 17-ketosteroids was essentially unchanged, thus ruling out the possibility that this is stress defined in the parameters of Selye. Although the exact mechanisms evade us, it is clear that a certain amount of sleep is a biological necessity. Working in a manner similar to starvation, sleep deprivation evokes a state of stress with marked inhibitory-conservatory manifestations and a parasympathetic shift.

MATERNAL DEPRIVATION IN THE NEONATE

In 1937 Margaret Ribble (59, 60) reported that neonates subjected to a mother's total rejection retreated into a comatose state. These mothers, because of immaturity and deep resentment, frustrated the infants' first attempts to suckle because of their lack of an erectile response of the nipples and clumsy handling. After two or three efforts at nursing, these babies suddenly became stuporous and developed Cheyne-Stokes respiration, extreme pallor and diminished sensitivity. It was as though these infants had regressed to a vegetative or umbilical level of function in which the sucking reflex is not necessary. They rarely wakened for feeding and when awake were listless and apathetic. Introduction of food into the stomach rarely relieved this condition. These infants had to be treated as though they were in shock. Rather drastic measures, such as saline clysis, intravenous glucose and blood transfusions were often required. After recovery they had to be taught to suck by stimulation of the mouth. Thus, the neonate responds to the mother's total rejection with a marked inhibition and depression of biological processes. If it were possible to demonstrate that the neonate is capable of forming mental representations of objects this phenomenon could be considered an example of response to object loss. The most likely explanation is a biological response to the lack of physical stimuli needed to maintain homeostasis in the neonatal period. However, as Engel (16) has pointed out, biological processes in action often precede the development of mental representations thereof. Thus, Ribble's coma of the newborn could be viewed as the biological anlage of processes which later will have psychological expression. In this restricted sense one may cite this phenomenon as an example of response to object loss in an

individual with an undeveloped or non-existent psychic apparatus.

OBJECT LOSS IN THE INFANT

Spitz (68, 69) reported on the effects of maternal deprivation on older infants with sufficient ego development to form mental representations of objects. He observed a number of normal infants with a minimum of six months, satisfactory relationship with their own mothers before they were deprived of them for varying periods of time without an effective maternal substitute. During the first month of deprivation the infants were irritable, weepy and demanding. In the second month there were weight loss and arrest of physical, psychological and social development. In the third month there were continued developmental retardation and weight loss, with a mute refusal of contact. These infants lay prone with averted faces fixed in a rigid expression. Although they were lethargic with restricted motility, the normal sleep pattern changed to a weepy insomnia. They were exceptionally prone to intercurrent ailments. Spitz calls this condition anaclitic depression, because of the similarity to clinical depression in adults.

There is a critical period of three to five months during which rapid remission can be produced by the return of the mother or an adequate substitute. If the critical period is passed these children become completely passive, withdrawn and unresponsive. When motility returns it is in the form of spasmus nutans and bizarre uncoordinated movements. The face is vacuous and uncoordinated eye movements contribute to an imbecilic appearance. Development deteriorates to a distressing degree and sitting, standing, walking and talking are not achieved even by the age of four. There is a progressive deterioration ending in marasmus and death. At the end of two years nearly 40 per cent of Spitz's subjects were dead in spite of adequate dietary and sanitary precautions.

The response of these infants to the loss of mother or an adequate object of gratification is, as with Ribble's neonates, a total psychosomatic reaction with marked inhibitory and depressive features.

OBJECT LOSS IN THE ADULT

Although grief is an experience as inevitable as anxiety there is surprisingly little to be found on this subject in scientific literature. The most important contributions have been made by Freud (25) and Lindemann (48). Grief, or the affect depression, is the emotional response to the loss of an object of gratification. Grief work is the painful ordering of the individual's psychic representation of external reality so that the image of the lost object is relinquished to correspond to the absence of the object in actuality. This is more than a simple readjustment to an environment in which the object is missing. It is emancipation from bondage to the lost object so that new relationships may be formed. Grief is too painful to be worked through in one Herculean effort, rather it is managed in recurrently intensified waves until the internal object has been dislodged from psychic reality and relegated to the realm of memories.

Grief is more than a psychic event. Like anxiety, it can be fully comprehended only as a psychosomatic reaction. There is somatic distress as well as psychic distress. The somatic distress reflects diffuse physiological disturbances and comes in waves lasting several minutes to an hour at a time. In addition to crying, disturbances in appetite and other bodily functions, there is a sensation of tightness in the throat, choking, with shortness of breath, need for sighing, empty feelings in the abdomen and lack of muscular power. Unfortunately, the physiological aspects of grief have been poorly studied as compared with the physiological aspects of anxiety and most of our data comes from subjective reports.

Many individuals find grief to be too painful to manage in a straightforward way. They avoid or defend against the grief work

with a variety of morbid reactions. There are individuals who postpone mourning only to have it appear inappropriately years later after a minor loss. Postponed grief may also manifest itself in anniversary depressions. Some individuals attempt to deny with a frenetic overactivity without a sense of loss. Although their activities have considerable meaning in terms of the deceased, they are unable to see this relationship. Others take on behavior and attitudes of the deceased, oftentimes very abruptly and to exaggerated degrees, to avoid bereavement. For them there is no reason to mourn because, through magic, they have retained the object within their own persons. This is a very dramatic example of what is meant by identification through incorporation and is a necessary step in the development of more complexly structured depressive phenomena. The acquisition of symptoms belonging to the last illness of the deceased can be a way of expiating guilt as well as avoiding grief work. Some individuals flee the environment of the object either physically or through marked alteration in relationships to friends and relatives who were also part of the object's world. Others become intensely involved with furious hostility against specific persons, often the physician who attended the deceased in his last illness. There are individuals who develop new behavior patterns which are detrimental to their social and economic existence. Greene (32) has recently described a proxy mechanism of mourning in which the individual assumes certain roles of the lost object and then treats another individual who has experienced the same loss as a vicarious substitute for the lost object in a manner appropriate for his deprived self, while at the same time avoiding grief work. More serious defense reactions are the severe reactive depressions and schizophrenic reactions. Most important for this discussion are the psychosomatic illnesses which develop in lieu of mourning. Lindemann was impressed that such diseases as ulcerative colitis, rheumatoid arthritis and asthma often appear as the morbid substitutes of normal grief.

A mature individual responds to object loss with a normal grief reaction. Those individuals who are immature or have defective psychic apparatuses defend against this painful experience with morbid reactions which may be mainly in the psychological and behavioral sphere with few somatic symptoms or with somatic pathology and minimal psychic discomfort.

DEPRESSION AND PSYCHOSOMATIC DISEASE

Lindemann's observations that certain somatic illnesses appear for the first time or are exacerbated during grief have been corroborated by many others. Engel (19) in his extensive studies of ulcerative colitis patients found that onset and relapses occur when there are threatened or fantasied interruptions of key relationships, accompanied by a sense of helplessness, despair and hopelessness. Chambers and Reiser (11), in a study of the precipitating events connected with episodes of congestive heart failure, concluded that loss of security through rejection by some key figure constituted an overwhelming threat which these patients responded to with cardiac decompensation. Stevenson and Duncan (70) reported on the influence of anxiety on diseased hearts, as well as a hypodynamic response evoked by stress interviews, which were reacted to with dejection, despair and anginal pain. These findings lend credence to folklore which insists that one can die of a broken heart. There are a number of other reports in the literature which deal with other disease entities, which seem to develop in a setting of real or fantasied object loss. These include diabetes mellitus (36), functional uterine bleeding (35), pernicious anemia (46), Raynaud's disease (54), rheumatoid arthritis (49), asthma (22), tuberculosis (13, 40, 75), thyrotoxicosis (42, 47, 56) and lupus erythematosus (50).

Kowal (44) in reviewing eighteenth and nineteenth century medical literature, points

out that physicians of these periods, who took a natural history approach to disease processes, were convinced of the relationship between despair and cancer. One of the most remarkable medical minds of the nineteenth century, Sir James Paget, emphasized the importance of depression as an etiological factor in neoplasms. These observations have a new timeliness in terms of the recent work of Greene (29–31). He finds that one of the multiple conditions determining the development of lymphoma or leukemia is the separation from a key object or goal, with ensuing atypical depression.

Benedek (4) has also emphasized depression as a basic psychosomatic mechanism. She postulates a depressive constellation of a basic psychobiological process arising out of disturbances in the early mother-child symbiosis. Later stressful life situations reactivate this depressive constellation with disturbances in both psychic and somatic spheres.

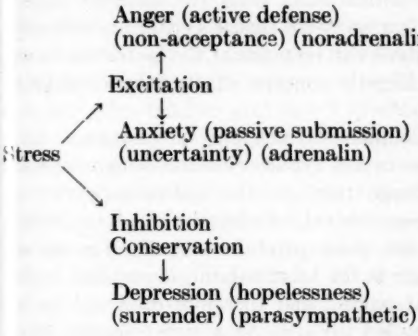
Schmale (63) has recently published a most interesting paper which supports Engel's concept of "separation-depression." He reports on an unselected group of semi-private hospitalized medical patients between the ages of 18 and 45 with a wide variety of medical conditions. Most of these patients had experienced a recent change in object relationships, actual or fantasied, which were interpreted to be unresolved at the time of onset of disease on the basis of continued feelings of helplessness and hopelessness.

DEFINITION OF STRESS

In view of the evidence cited, it seems reasonable to postulate that stress produces psychobiological states of either excitation or inhibition-conservation. Either physiological or psychological stimuli are capable of evoking these states. At a biological level agents which act directly to produce tissue damage stir the individual to a state of excitation. The state of inhibition-conservation is generally the result of depriving the organism of some necessary substance, stimulus or experience needed to maintain homeostasis. Also, as Kubie (45) points out, there are anticipatory intra-organismic signaling mechanisms which regulate instinctive responses. At a biological level these responses can be set into action in "anticipation" of a danger or a need which is as yet not a biological reality or has as yet no psychological representation. For example, we breathe not because we are suffocating but because of signals set off which "anticipate" anoxia if we didn't breathe. Pain may signal actual tissue damage or merely the danger that it may occur. Hunger is a signal triggered in anticipation of tissue deprivation. The onset of adverse environmental conditions may evoke dormescent states in animals which hibernate or aestivate whereas closely related non-hibernating animals may respond with frenetic hyperactivity. Serious organic illnesses may evoke physiological or psychological signs of either excitation or depression long before a diagnosis is possible.

The psychological stimuli which evoke a state of stress arise from situations which the individual interprets to mean that a source of gratification either is threatened or has been lost. If the individual feels his source of gratification is threatened the response is excitatory (fight or flight). If the individual passively experiences this state of excitation it is called anxiety. Should the excitatory state be actively directed against the source of danger it is called aggression (rage or anger). If the individual feels he is deprived of gratification the response is depression (grief). Anxiety, aggression and depression are psychobiological states with characteristic patterns of autonomic activity. Anxiety and aggression are expressed mainly through adreno-sympathetic mechanisms. Anxiety is accompanied by an adrenalin pattern of autonomic activity and aggression with a noradrenalin pattern (2, 27, 66). Depression is expressed physiologically, at least in part, through para-

sympathetic mechanisms. Stated schematically:



DISCUSSION

The thesis presented here is that anger, anxiety and depression represent basic psychobiological states from which the other emotions of displeasure are later differentiated. In the neonate these states are expressed as innate, raw physiological discharges without psychological representation. That is, they are emotions without objects. With the maturation of the psychic apparatus and the formation of early object relations these innate biological processes become integrated into a total reaction with psychological representation thereof. It is at this point that one may speak of them as basic ego reactions. Bibring (5) postulates basic ego reactions of normal balanced narcissism and elation as well as anxiety and depression. He does not include anger. However, the neonate seems to be as capable of anger as of anxiety. Sham rage and similar phenomena demonstrated in animal preparations (65, 67) may be cited as evidence that anger is a biologically basic reaction. Ictal affects most frequently reported are those of anxiety, depression and elation (12, 74). Anger is less frequently reported. Greenson (33) is in agreement with Bibring that depression is a basic ego reaction. Frank (21) also views depression as a basic psychobiological reaction and emphasizes the point that depression is more than mere retroflexed or dammed up aggression. To entertain such a view would be comparable to holding to Freud's original but later abandoned idea that dammed up libido turns into anxiety.

Perhaps the best way to evaluate ego states is in terms of the individual's object relations. Szasz (71) defines an object as any system with which the ego establishes a mutual interaction. Objects vary with the developmental state of the ego and may be such different things as mother's breast, mother as a person, the church, God, image of a deceased loved person, social organizations and institutions, the body, or some bodily part. This very inclusive concept of objects permits a simpler solution of the problem of the relation of castration anxiety to separation anxiety than Freud postulated in his latest formulations (24). Freud accepted Ferenczi's postulation that the penis is a highly valued organ because possession of it is a guarantee of reunion with mother or a mother substitute in the act of coitus. Therefore, loss of the penis is tantamount to a second separation from mother and castration anxiety is a derivative of separation anxiety. According to Szasz, the ego may relate *vis-à-vis* the body or a part of the body in the same manner as it does to mother. Thus, castration, separation or social anxiety in the adult may be defined as the passively experienced reaction to a danger, real or fantasied, present or anticipated, which threatens the loss of an object of gratification. Anger may be defined as the active efforts to eliminate or destroy the danger which threatens the loss of an object of gratification. The emotion of depression (grief or mourning) is the reaction to loss, actual or fantasied, of an object of gratification. Anxiety and anger are always anticipatory phenomena even though they may be the reaction to a present danger. Depression is a consummative phenomenon from which there is no reprieve. One may feel helpless in the face of anxiety but one is not without some degree of hope. In the

face of depression one is not only helpless but also despairs of hope for the return of the object. Perhaps this is the answer to Freud's question of why the especial painfulness of mourning (24).

In understanding the emotions of anxiety and depression in terms of object relations, one must evaluate events in terms of the internal object because what an impartial observer may conclude from his observations of the external object may not coincide with intrapsychic events. The observer may interpret events to constitute a threat of object loss, yet the individual reacts as though he has lost the object. The source of these reactions is not so much what happens to the external object but rather the way in which the internal object is manipulated. If the individual cannot order external stimuli in terms of past experiences he may arrive at faulty conclusions regarding the state of his object relations. This may result from the actual distortion of the stimuli or pathological endopsychic processes. An extreme example is that of the wives who had genuine and complete grief reactions when their husbands went into the armed forces. When their husbands returned they were unable to continue with their marriages because they had relinquished their husbands as loved objects (48). Other women responded to the same situation with anxiety or phobic reactions. More commonplace are those instances of slight rebuff or inattention, which are taken as evidence that the loved or needed one is lost. I am reminded of a woman, with ulcerative colitis in remission, who had an exacerbation when her usually punctual husband was 15 minutes late for dinner one evening. No amount of reassurance could convince her that he had not withdrawn his love and she rapidly went downhill and expired from a perforation in a week's time. Thus, it can be seen how separation or threat of separation from an external object may cause anxiety on one occasion and depression on another, which is the question Freud posed in the addendum to "Inhibitions, Symptoms and Anxiety." One can predict the reaction only if the interactions with the internal object are known. With this as a criterion, a sharper analysis can be made of the factors included in Engel's concept of "separation-depression."

Complex structuring of responses into more or less typical clinical entities depends in large part on the individual's use of various secondary affective states to defend against these painful basic affective states. Anger is the least painful; depression is the most painful and therefore most apt to be defended against. At a psychological level these basic affects may be repressed or masked more or less completely by counterphobic defenses. However, complete masking or neutralization does not occur at a physiological level. The basic autonomic pattern remains but is modified by the emotions evoked to defend against the basic state Structuring also arises from the fact that at any one point in time an individual interacts with many objects. The main character of reactions is determined by the major object relations with modifying influences stemming from minor object relations. Thus it can be seen that a rather complex structuring of both physiological and psychological responses does occur.

THE PROBLEM OF PANIC

The parasympathetic discharges observed in panic states are often cited as evidence that intense anxiety may be expressed through parasympathetic channels. However, panic is more than anxiety as defined above. Panic is a special condition in which the individual progresses from a state in which he perceives a major object to be in grave danger to one in which he admits the loss of this major object. In this progression there is an intermediate point at which intense anxiety and depression overlap. It is at this point that the individual experiences a traumatic separation from the object. The blending of intense anxiety and depres-

sion produces a state of panic which is characterized by massive sympathetic and parasympathetic activation. An example is the soldier who becomes overwhelmed by the increasingly intense stimuli of battle and panics with all the indications of intense anxiety plus bladder and bowel evacuation or erection and ejaculation. Similar panic states may result when faulty conscious or unconscious endopsychic mechanisms lead the individual to feel that a major object is in increasingly grave danger and finally has been lost to the individual. When minor objects are involved the psychological and physiological responses are proportionately diminished in intensity except in those instances where the minor object symbolically represents a major object.

IMPLICATIONS FOR RESEARCH

If the unitary theory of emotions which designates anxiety as the basic or primary emotion of displeasure is replaced with a multiple theory, a re-examination of psychodynamic formulations and nosological groupings will be necessary. Greenson (33) has done some preliminary work along these lines using a concept of primal depression as well as primal anxiety. He attempts a regrouping of various clinical entities in terms of their derivation from primal anxiety or depression. Additional work of this nature is needed, particularly in the area of psychosomatic illnesses and depressive reactions. It seems a matter of great importance whether depressive reactions are derived from primal anxiety or primal depression.

Perhaps the main reason for our lack of data on the psychophysiology of the emotion of depression or grief is the great difficulty in producing grief in either animals or humans under laboratory conditions. To get these data we will have to take our laboratories to the grieving person. Although the technical problems will be great they will be dwarfed by the personal, social and cultural obstacles raised in protest.

Engel (17) views grief as a disease. The position taken here is that a mature individual is capable of doing grief work which, although painful, does not constitute physical or psychological pathology. The immature individual defends against doing grief work with mechanisms that result in psychological and/or physical pathology. However, the question of whether grief is always a disease or may be a "normal" experience can be settled only by further careful studies.

CONCLUSION

In summary, the concept of stress has been extended to include inhibitory-conservatory shifts as well as excitatory shifts in homeostasis. These shifts may be either adaptive or maladaptive. If the response is prolonged in duration or inappropriate to the stress, pathophysiological and tissue changes may result.

Correlated with these shifts in homeostasis are characteristic affective responses which may be viewed as basic ego states. Anxiety and anger are characteristic of excitatory shifts; depression, of inhibitory shifts.

These basic psychobiological reactions may be triggered by either biological or psychological stimuli or forces. At a biological level, agents which produce tissue damage evoke a state of excitation (anxiety or anger). Inhibitory reactions (affect of depression) are the result of deprivation of some necessary substance, stimulus or experience. The psychological stimuli which evoke a state of stress arise from situations which the individual interprets as indicating that an object of gratification either is in danger (anger and anxiety) or has been lost (depression).

These basic psychobiological reactions are blended and structured in various ways into more or less typical clinical entities.

There are large gaps in our knowledge of grief and depressive phenomena. Psychophysiological, clinical, sociological and anthropological research in these areas is

needed and should explain phenomena which are presently not understandable in terms of Cannon's emergency theory, Pavlov's conditional reflex physiology or Selye's general adaptation syndrome (18). There is a great need for more knowledge of anxiety, anger and depression as basic psychophysiological expressions of object relations and the emergence and maturation of other psychophysiological mechanisms as the individual progresses from infancy through adulthood.

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A PAVLOVIAN APPROACH TO SYMBIOTIC PSYCHOSIS

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INTRODUCTION

Between 1949 and 1955 Mahler and her co-workers (5–7) described a variety of childhood schizophrenia for which they proposed the name of symbiotic psychosis. Although accepted by some as a valuable distinction (by Kanner [3], for example), others have found such difficulty with it that they have argued against its acceptance as a separate variety (1). This paper examines the disorder and attempts to evaluate the usefulness of Mahler's distinction. It proposes a restriction of the use of the term and outlines a Pavlovian approach to the problem. It is argued that this restricted definition provides a more scientific explanation than does psychoanalytic theory.

Mahler (5) defined the symbiotic psychoses as those "in which the early mother-infant relationship is marked but does not progress to the object-libidinal cathexis of the mother. The mental representation of the mother remains or is regressively *fused with* —that is to say, is *not* separated from the self." Following a more or less normal first year of life, the disorder develops when the illusion of symbiotic omnipotence is threatened. Severe panic reactions occur and the symptomatology which manifests itself is one which serves to maintain or restore the fusion with the mental image of the mother or father or condensed mother-father image.

The present evaluation of the concept will be made under the headings of symptomatology and diagnosis, etiology and treatment.

SYMPTOMATOLOGY AND DIAGNOSIS

Although onset after the first year of life is always a necessary feature (generally occurring between two and one-half and five years), the symptoms need not include severe panic reactions followed by attempts at reunion with the mother image. Mahler and Gosliner (6) have pointed out that regression can take place to a stage such that "the patient may no longer show any sign of missing his real mother. It is at this stage that these children are routinely misdiagnosed as autistic cases." Cases of symbiotic psychosis do not exhibit the same uniformity of symptoms as do cases of early infantile autism but this is not, as Despert and Sherwin argue (1), the main reason for difficulties with the concept. The main reason is to be found in the fact that Mahler uses the term symbiotic psychosis to apply to all cases of childhood schizophrenia with the onset in the second to fifth or sixth year of life.

Originally Mahler, Ross and De Fries in 1949 (7) distinguished the following three groups of childhood schizophrenias:

Group 1. Cases showing an arrested development of the ego system.

Group 2. Cases showing a regressive disintegration of the ego system.

Group 3. Cases where more or less large parts of the personality were retained intact.

These three groups of cases appear to cover the whole field of childhood schizophrenia. After equating (incorrectly, in the writer's view) Kanner's early infantile autism with the Group 1 psychoses, Mahler noted that the symbiotic psychoses were "described in a previous paper as Group 2 of 'schizophrenia-like' clinical pictures in children" (5, p. 297). Thus all regressive psychoses in the pre-school child are covered by the term symbiotic. (The Group 3 cases are distinguished by less disintegration and by later onset.)

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Now, since the specifically symbiotic symptoms may not always be present, the usefulness of the concept hinges upon the presence of specifically symbiotic etiological factors in *all* such regressive disorders. Examination of the etiological factors suggested by Mahler is therefore necessary.

ETIOLOGY

Defects in the resolution of the mother-child relationship at the time when the child normally becomes independent of the mother are held to contribute to the disorder. Together with this a constitutional factor is involved and, as will be seen, is given greater weight than the former.

While in the second of the papers under discussion Mahler says that it is "very difficult to ascertain whether the grave disturbance . . . has been caused by the mother's pathology and lack of empathy, or by the infant's great innate ego deviation" (5); in the third paper she regards the mother's role as only an "auxiliary" one (6). Of the children she says that they "are constitutionally vulnerable and predisposed toward the development of a psychosis. It is the very presence of the constitutional ego defect in the child that helps create the vicious circle of the pathogenic mother-child relationship by stimulating the mother to react to the child in ways that are deleterious to his attempts to separate and individuate."

Concepts of constitutional defect usually involve difficulties. Mahler's presents more than the usual quota since she does not define it adequately or show precisely how it contributes to the disorder. Thus the constitutional ego defect underlying a symbiotic psychosis is said to be "an inordinate need for symbiotic parasitic fusion with the adult" (5, p. 289), as well as a "proclivity to ego fragmentation" (5, p. 285). While there may or may not be a contradiction between these two rather distinct notions, they are not easily related to the only objective sign of such defect which Mahler mentions: that of the hyperkinesis and hypertonicity displayed by Aro (6, p. 202). Neither is it apparent from her account how these particular characteristics contributed to Aro's disorder.

Contrasted with the symbiotic child's need for fusion is the autistic child's "inherent lack of contact with his living environment" (5, p. 289). Consequently, both of these assumed needs must be present in the many cases exhibiting mixed symbiotic and autistic features, particularly where symbiotic features have developed out of an earlier autistic psychosis. Logically, this seems indefensible.

Even if Mahler's use of the concept was restricted to the unmixed symbiotic psychoses, it is to be noted that the constitutional ego defect can manifest itself even when the mother has not been exceptionally possessive, infantilizing and restrictive (5). Additionally, behavioral data pointing to disturbances in the resolution of the symbiotic relationship, while the relationship was actually being resolved, are not usually found. Mahler's view is that the main condition for the appearance of such behavior—the differentiation of self from non-self—is not present at that age (5, p. 297). Thus the role played in the production of the psychosis by disturbances in the resolution of the symbiotic phase is based only upon an inference from the presenting symptoms and is, in any case, limited to determining the content of the disorder. Under these circumstances the classification of a psychosis as "symbiotic" can contribute little to an understanding of the genesis of the disorder.

TREATMENT

The distinction between the therapeutic regimens for the autistic and symbiotic cases which Mahler has outlined is of considerable importance (6). But as this distinction can be based upon the symptomatology alone (and certainly deduced from a quite different etiological standpoint), the specificity of the symbiotic program cannot

be considered relevant to the question of etiology. That is to say, while the differential treatment suggested is consistent with the proposed etiology, it is actually independent of it, and cannot be used to support her theory of symbiotic psychosis.

SUMMARY OF CRITICISMS

To summarize, it can be said the concept of the symbiotic psychosis is an overinclusive one in that it refers to all cases of regressive psychosis in the pre-school child; that, since its cause is said to be a constitutional ego defect (and this concept itself involves major difficulties), additional defects in the resolution of the symbiotic relationship can, at most, determine the content of the disorder; and that, because the concept is a low order inferential one, it contributes little to an understanding of the genesis of the disorder.

RESTRICTED USE OF TERM

The writer believes that the concept can be retained only to the extent that these criticisms can be overcome. This would be achieved by restricting the concept to those psychoses which are:

1) conditions of arrested development manifesting themselves during the second or third year of life and characterized by an irrational, panicky anxiety occurring in the context of a continuing emotional interdependence of mother and child; and
2) regressive conditions in which behavioral data indicate that defects in the resolution of the symbiotic relationship play a definite causal role in the production of the psychosis.

Several advantages would follow from such a use of the concept. While in all cases symbiotic features are to be expected in the symptomatology, a clear distinction (not evident in Mahler's work) would be made between conditions of arrest and conditions of regression. It might be expected that the therapeutic tasks in each case would be different. Another expectation might be that causal influences could be traced in some detail and constitutional factors given a more scientific basis.

Finally, one would not necessarily be committed to the Kleinian theory of psychic development which is obligatory if Mahler's view is followed. More particularly, from the writer's viewpoint, these cases would be explicable with Pavlovian concepts. This will be touched upon in the discussion of cases selected to illustrate the proposed restricted usage of the term symbiotic. After outlining some of the basic concepts of Pavlovian theory, I will apply these concepts to two cases, one of arrested development and one of a regressive condition.

PAVLOVIAN THEORY AND SCHIZOPHRENIA

From the Pavlovian viewpoint, schizophrenic symptoms are seen as a consequence of a state of cortical inhibition. Ignoring the question of auto-intoxication, one can say that there are broadly two sets of conditions which bring about states of cortical inhibition—breakdown conditions and conditions of faulty learning. The former are so well known from the literature on experimental neurosis as to need only the comment that while the form of the breakdown depends to some extent upon the methods used to produce it, the main determinant is the type of higher nervous activity.

Three properties of the cortical processes of excitation and inhibition are used to establish the type: strength, balance and mobility. On the basis of the first criterion the processes are categorized as strong or weak; on the basis of the second, the strong types are divided into balanced and unbalanced (usually with excitation predominating over inhibition); and on the last criterion the strong, balanced type is further subdivided into the labile and inert. Procedures used in establishing type are fully described by Pavlov (10, pp. 313–342). Currently the following four types are recognized.

1. The weak type, in which inhibition predominates over excitation.
2. The strong, unbalanced type in which excitation predominates over inhibition.
3. The strong, balanced type with labile cortical processes.
4. The strong, balanced type with inert cortical processes.

In man, the typology is complicated by having to consider three possible modes of first and second signaling system interaction in each of the four general types (10, pp. 481–486). Although in the child, particularly the pre-school child, speech is of considerable importance in directing behavior, the second signaling system is so little developed that the question of its predominance or otherwise over the first signaling system hardly arises. Because of this, and because the child's behavior can usually be understood fairly adequately on the basis of his general type, the specifically human type has been largely disregarded in this article.

The properties on which the typology is based are usually considered to be innate, but alterable by training. While the writer has seen no experimental evidence to support the latter opinion, it is true that training procedures which can be described as faulty learning situations can produce a state of affairs in which the type is masked. The famous experiments of raising puppies in isolation are the most important evidence in favor of this. Pavlov observed that, although these animals were of strong type, the passive defensive reflex exhibited in all new situations brought about such a pronounced inhibitory state (through the mechanism of negative induction) that superficially they behaved like the weak types (10, p. 317; 2, p. 90).

Hence it seems obvious that inhibitory cortical states may result from a breakdown, or by a process of negative induction from subcortical excitation. This latter mechanism develops in those cases where both simple isolation and continued reinforcement of sub-cortical processes operate. Both of these conditions may be categorized as faulty learning conditions. An example of the consequences of simple isolation is to be found in an earlier paper by the present writer (4). An example of the latter is given in Case 1 below.

Breakdowns of a more noticeably inhibitory nature occur with all types, especially the weak. Not so obvious is the fact that inhibitory phenomena are to be observed when the strong, unbalanced type breaks down. Most often excitatory phenomena are then observed; but, since the cortex can rarely maintain a permanent state of excitation, inhibition supervenes. This fact is emphasized here only because the illustrative Case 2, below, has all the features of a breakdown in a strong, unbalanced type.

CASE 1: SYMBIOTIC PSYCHOSIS AS ARRESTED DEVELOPMENT (FAULTY LEARNING TYPE)

Dennis was referred to the clinic at the age of four years and eight months. The accompanying letter from the kindergarten stated that he had been found to be "different from the ordinary new child." When speaking or spoken to, he would focus on some distant object, and anything that was said to him was merely repeated by him . . . he showed no interest in the toys . . . while a story was being told, Dennis would walk round and round in circles. . . . kept repeating, "Up the hill, down the hill and round the corner" (in reference to the bus trip). The clinical impression was similar and, together with the mother's story, provided the following symptomatology: unresponsiveness, seclusiveness, excessive fears (mostly of new situations and especially of separation from the mother), disturbed sleep, and speech disorder (echolalia, phrase repetition, referring to himself in the third person). Developmental milestones were: sitting at six months, walking at 15 months, talking with single words at about 18 months, and sentences after two years. The possibility of mental retardation had occurred to the mother, but on psychometric examination he obtained formboard scores at or above his age level.

Marital relations between the parents had been strained from the beginning. The father had reluctantly entered a valid mixed marriage with his Catholic wife, nearly breaking the engagement when he realized the religious implications. On the honeymoon the mother developed vaginismus, the father reacted to this by calling her "a freak,"

and resisted attempts to discuss the problem with a Catholic marriage guidance agency. Three months after the marriage he began to explore the possibilities of a divorce, and in the second, third and fourth years of the marriage the parents lived apart, seeing each other only at weekends. Opposed to birth control methods, the father even refused to use the "safe-period" method despite (or because of) mother's acceptance of this. Dennis was conceived four years after marriage. By this time, the mother had dissociated herself emotionally from the father. Similarly, the father did not visit the mother while she was in the hospital for her confinement. Even during Dennis' first year, the father lived at home only on a temporary basis. From the beginning the mother noticed that Dennis was "different" from other babies. "He seemed to need me so much. He was always crying at feeding time—a desperate panicky cry." In the counseling sessions later, she said that she had sought, and obtained, emotional satisfaction from the child because "if you don't get it from your husband, you'll look for it somewhere else." Emotionally isolated from her husband, it is not surprising that her feelings and anxieties became transmitted to the child, especially when, at seven months, he had a convulsion while teething. He had two more convulsions, the last one at two years (associated with a middle ear infection) being serious enough to require hospitalization. The child's psychotic characteristics were intensified by this experience but, after a few days, "he settled down to what he was like before." The mother was quite emphatic that there had not been a period of normal development preceding the appearance of the psychotic condition, which seems to have become noticeable by 18 months. The only notable features in the family history were a traumatic epilepsy contracted late in the maternal grandfather's life and some possibly obsessional features in the paternal grandmother. Definitely only the first of these was relevant, and this only because it added to the mother's anxiety at the time of Dennis' convulsion.

Although the mother had initially reported the baby's condition after birth to be normal, it transpired, during therapeutic sessions with her, that the child had been mildly jaundiced for a short time after birth. The doctor who delivered the child informed the clinic that this was not due to an Rh incompatibility, and added that the child had shown signs of cerebral irritability. Examination at the time had failed to disclose the cause and, in any case, the child's later development did not reveal any sequelae of either the jaundice or the irritability.

DISCUSSION

Even superficially there seems to be a close connection between the child's symptoms and the mother-child relationship. Ready to use the child as an emotional substitute, herself anxious and uncertain as to the future of her married life, the conditions for the mother's reinforcement of the child's anxieties, rather than their dissipation, were present from the beginning. Interpreting the child's cry in this context, we find the conditions realized.

The following Pavlovian mechanism is postulated to account for the gradual development of the psychotic condition. The fear and anxiety would be seen as an expression of the passive-defensive reflex, and this typically leads to a state of cortical inhibition by a process of negative induction. A permanent inhibitory state would account for the inadequate learning, the speech and sleeping disorders and, especially, the unresponsiveness. For the passive-defensive reflex to produce such a permanent inhibitory state would require the assumption of its continuous reinforcement (*cf.* 2, pp. 264–286). This would be a consequence of the mother's continued anxiety and dependence. The initial appearance of a reflex of such strength could also follow from this, although the possibility of its being an innate quality is equally likely. The unknown cerebral condition might also have played a direct role, but this is not a necessary assumption.

In order to overcome possible bias in the selection of cases, the liberty is taken of reinterpreting in Pavlovian terms a regressive disorder reported by Mahler, that of Aro.

CASE 2: SYMBIOTIC PSYCHOSIS AS REGRESSIVE DISORDER

Aro's case is fully reported by Mahler in her third paper (6) and the basic data will not be repeated here.

From Pavlovian theory, early symptoms of hyperkinesis and hypertonicity can be shown to have an intimate connection with the disorder which eventually developed. These symptoms suggest a predominance of the excitatory process over the inhibitory. Experimental work in animals has shown that where such a predominance exists, inhibitory (negative) conditioned reflexes are difficult to elaborate. Differentiation, or discrimination, between positive and negative stimuli also

presents difficulties, and even simple dynamic stereotypes (complexes of positive and negative reflexes), if elaborated, are unstable (10). Pavlov uses the following words to describe the behavior of such animals: lively, active, aggressive, excitable, animated, unrestrained, undisciplined, losing control easily (8, pp. 334, 343, 375-376; 9, pp. 105, 107).

Applying these findings to Aro, we note, first, that from an early age Aro's behavior was similar (hyperkinesis, aggression). His temper-tantrums and, possibly, the toilet training difficulties are consistent with difficulty in elaborating inhibitory conditioned reflexes. (The pyloric stenosis also suggests an inhibitory failure.) His mother's attitude changed abruptly and radically when she sided with the siblings against Aro, a perfect example of a situation calling for changes in response from positive to negative. The mother, as a stimulus, seems to have had contradictory qualities which would prove difficult to differentiate; for example, in the feeding situation. (This possibly applies also to the nurse.) The only uncomplicated, stable stimulus appears to have been the grandfather, and it is significant that Aro was always more responsive to him and that it was the grandfather's death which precipitated the tics. The tics themselves, although possibly based originally on a conscious action, maintained their compulsive character because of an inertness of the excitatory process. Such pathologically inert states are a common consequence of the breakdown of a dynamic stereotype (11, p. 303). Briefly it can be said that the neurotic condition developed as a consequence of the demands made upon this particular type of nervous system. Hence Pavlovian theory allows the tracing of a closer connection between the events in this pre-psychotic phase and the constitutional factor than does Mahler's rather vague theory.

Insufficient data are available to determine whether Aro's dependence was learned directly from the mother or was part of a general weakening of the cortical processes following feeding and toilet traumas.

The precipitation of an overt psychotic picture was due to various fear-provoking stimuli. The picture itself has many of the characteristics of a full-scale breakdown shown by experimental animals in which excitation predominates over inhibition. In these, inhibitory stimuli become positive (and *vice versa*), states of excitability alternate with inhibitory states, established differentiations disappear and the ability to analyze and discriminate among stimuli is lost (8, pp. 334, 343, 351, 359, 389; 9, pp. 64, 84, 92, 105, 180). Aro's ambivalence toward his mother, here considered a part of the ambivalence common to all his symptoms, is strikingly similar. His inability to analyze and discriminate among stimuli, animate and inanimate, is accounted for as are the "frankly psychotic" outbursts.

Events in both the neurotic and frankly psychotic phases of Aro's illness are closely related to his higher nervous activity, in which excitation predominated over inhibition. A further intimate connection between the psychotic character of the disorder and the constitutional factor is thus suggested.

Furthermore, it may be speculated that, had Aro's initial difficulties been handled differently (*i.e.*, in such a way as to strengthen gradually, rather than to weaken, an already relatively weak inhibitory process), the constitutional factor would not of itself have produced the psychosis. While Mahler's theory implies the inevitability of this, Pavlovian theory would seem to offer greater preventative and therapeutic possibilities.

CONCLUSIONS

It has been argued that Mahler's concept of symbiotic psychosis is an overinclusive one, and that her theory lacks explanatory power. Two types of symbiotic disorder have been distinguished: conditions of arrested development and regressive conditions. Pavlovian theory has been held to satisfactorily explain both types of disorder at least as well as Mahler's theory. Insofar as the role of constitutional factors is evaluated more clearly, it is submitted that the Pavlovian explanation is superior.

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THE INFLUENCE OF VARYING DOSAGE ON THE EFFECTS OF LYSERGIC ACID DIETHYLAMIDE (LSD-25) IN HUMANS

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INTRODUCTION

The relationship between the dosage of LSD-25 and the magnitude of the psychophysiological response in humans has not been fully studied. Many contradictory statements are found in the literature. For example, Rawnsley and Anderson (4) state, ". . . over a certain minimal dose there is no clear-cut relationship either qualitative or quantitative between the clinical picture and the amount of substance taken." In contrast, Abramson *et al.* (1), using a questionnaire, found that with increasing dosages, from 0 to 225 micrograms of LSD, the number and subjective severity of symptoms increase. Isbell *et al.* (2) have confirmed these results up to 2 micrograms/kg., using Abramson's method.

It would be surprising if there was not a close relationship between the dosage of LSD and its effects. In our own work with LSD in the course of other studies we observed considerable variations in the effects of the same dose in different subjects, but a relative consistency of effects when the same dose was repeated in a given subject. Thus our gross observations were in accord with the findings of Abramson and of Isbell. We felt, nevertheless, that additional detailed and systematic studies were needed to study further the dose response relationship, especially in the higher dose range. This is important for several reasons. From a pharmacological point of view LSD is most interesting in producing unique and powerful CNS effects in minute doses. Its mechanisms of action, however, have remained obscure. Any peculiarities in the relationship between dose and response should have direct relevance to the search for mechanisms of action. From the point of view of those involved in studying the psychological effects of LSD it is most important to know the relative effects of varying dosage. Confusing and ambiguous results might easily occur if the investigator were ignorant of them.

The present paper describes two separate studies dealing with the dose response relationship of LSD. Both studies are within the same framework and follow the same design.

PROCEDURE

Sixty normal male subjects between the ages of 19 and 40 years were screened by psychiatric interviewing. Of this number, two groups of six subjects each were selected, one for each of the two studies. Subjects were selected for the qualitites of "good adjustment," cooperativeness and ability to express themselves verbally. These qualities were determined entirely on a clinical basis by the psychiatric interviewers. Good adjustment here refers to the absence of any obvious psychopathology. In making this judgment the interviewers considered such factors as social adjustment and neurotic symptoms, as well as the more subtle elements of interpersonal behavior as noted during the interview.

In the first study, LSD was administered

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orally in distilled water, in a logarithmic series of 1, 2 and 4 micrograms/kg. In the second study, a series of 4, 8 and 16 micrograms/kg. was given. Each subject received all three dose levels of a series on separate days, one week apart.

On each experimental day, there were two subjects at each dose level. Within these limits the doses were administered in all possible orders and sequences. The two studies, therefore, included 12 subjects, each receiving three doses of LSD, for a total of 36 separate doses. The maximum total dose administered to one subject, on the basis of 16 micrograms/kg., was 1654 micrograms. Administration was double-blind with respect to dosage. The schedules (with table entries in micrograms/kg.) were as follows.

Series #1

Subjects	Experimental Days		
	1	2	3
A	1	2	4
B	1	4	2
C	2	1	4
D	2	4	1
E	4	1	2
F	4	2	1
Series #2			
G	4	8	16
H	4	16	8
I	8	16	4
J	8	4	16
K	16	8	4
L	16	4	8

Considerable effort was made to provide a supportive environment. Before the studies were begun satisfactory relationships were established between all observers and subjects. The subjects were given an opportunity to become acquainted with one another and with the ward setting where the experiment was conducted. During experimental days an effort was made to continue good rapport and to provide a friendly atmosphere. Within the limits of the unpartitioned ward, subjects could move about freely if they chose or lie in bed (as most of them preferred to do, especially on higher doses). When not being interviewed they were allowed to converse among themselves. Between experimental days subjects were interviewed several times and encouraged to air any questions they had. Although the subjects were often reminded that they could drop out whenever they wished, none chose to do so.

Subjects were rated by three medical observers, each of whom had previous experience with LSD subjects and with the system of rating used. Two of the raters were psychiatrically trained, while the third was trained in internal medicine, but had one year of experience in working with the same psychiatrists, performing various studies of humans with LSD-25. Each subject was interviewed three to five times by each observer on an experimental day, and by at least one observer on the following morning. Between interviews, the subjects were under continuous observation by all three observers while confined to the ward. Interviewers worked independently of one another. Every interview was begun with a brief period of unstructured interviewing, with the purpose of eliciting any material not covered in the structured part of the interview which followed. For the structured part of the interview, results on each subject were recorded on a rating sheet listing a large number of physiological and psychological variables. These included pupillary dilatation, palmar sweating, perceptual changes, memory, ability to concentrate and to abstract, alterations in time sense, affect, feelings of depersonalization, delusion formation, among others. Ratings were made on a scale of 0 to 4+. In addition, the interviewer would make notes of any other noteworthy findings. All observations were made clinically, but with an effort to be objective. For example, the ability to concentrate was based both on overall interview behavior and on the subject's ability to perform mental arithmetic or other relevant tasks.

The clinical interviewer would assign ratings according to the apparent degree of change or impairment, compared with the subject's non-drug state. In addition to the interviewers, two other physicians and a technician had the responsibility of making detailed physiological observations. These data were not available to the interviewers until after they had completed their ratings of severity of response, following the experiment.

By this method, and with the experimental design which we have described, it was possible to compare subjects against one another and to compare each subject against himself at the various dose levels.

At the conclusion of both studies, all observers independently prepared tables in which they attempted to guess the days on which each subject got each dose. Since the observers were familiar with the outline of the design followed, their task was to assign to each subject a letter A to F, or G to L, in accordance with the design. No attempt was made to arrive at an evaluation solely on the basis of the scores recorded on the rating sheets during interviews. These data were not sufficiently precise for this purpose. Instead, each interviewer studied his ratings, considered his notes, reflected on the total experience, and then arrived at his conclusions.

RESULTS

Quantitative Results: The reliability of the observers' judgments in both series was perfect, *i.e.*, the schedules drawn up by the three observers at the end of the experiment were identical. Moreover, the validity of the judgments was also perfect, *i.e.*, the schedules duplicated the one which was actually followed. For each individual rater this event would be expected to occur only once in 720 times on the basis of chance alone (p = .0014). For all three raters to have hit upon this by chance alone the probability is one in $(720)^3$. The results were the same in both series.

Clinical Reactions: We shall not attempt to describe most of the clinical phenomena which were observed, or to dwell upon the unique occurrences. For our purposes, it is more relevant to deal with certain aspects of the LSD reaction which were qualitatively much alike for most subjects and which were among our chief criteria for discriminating between doses.

In all subjects, the LSD produced changes in thought processes and perception. Physiological effects, chiefly in the form of an autonomic discharge, also occurred in every case. These effects were usually relatively mild with the lowest doses and proportionately more severe with increasing doses for all subjects. At 16 micrograms/kg., effects in all areas were profound, yet qualitatively the psychological effects were no different from those at lower doses. We shall describe separately the changes in each of the three areas.

At 1 microgram/kg. subjects had impaired memory for recent events and difficulty in focusing thoughts as well as impairment of abstracting ability, as determined clinically by proverb interpretations. Serial 7 subtractions were usually done somewhat slowly and inaccurately. With increasing dosage, these functions were proportionately impaired. At 16 micrograms/kg. subjects could not repeat the words of a short sentence and serial 7 subtractions could not be performed at all. Proverb interpretations were progressively more difficult and more concrete with increasing doses. At 16 micrograms/kg. subjects could usually not even attempt them. The tendency to withdraw into autistic fantasy with vivid visual imagery also increased with greater dosage.

Two subjects developed paranoid ideation and several other experienced mild depressions. These reactions occurred each time these subjects received LSD. They seemed to be related more to individual predispositions than to dosage.

Visual perception was affected in varying degrees at all doses. At 1 microgram/kg. subjects experienced visual distortions and

hallucinations. Distances, sizes and shapes of people and objects would change in an unpredictable manner. Flashes of lights and colors were seen. No hallucinations of formed objects were reported at this dose, except when subjects shut their eyes. With larger doses, changes of the same sort occurred but were far more intense. Almost continuous hallucinations of colors, patterns and objects were seen at 16 micrograms/kg. At this dose, one subject reported seeing, while his eyes were open, "horrible green-eyed monsters" everywhere he looked. At higher doses, subjects also had more difficulty in discriminating between hallucinated and real objects. In all cases, hallucinatory images were more intense with the eyes shut.

Somesthetic perception was also altered at all doses. Subjects reported sensations of numbness and paresthesias throughout the entire body, as well as bodily estrangement and distortions of body image. These effects were experienced even at 1 microgram/kg. but were comparatively mild at this dose. At 16 micrograms/kg. all the symptoms were most severe. Bizarre explanations were often found for the odd sensations experienced. For example, one subject reported a constant flow of electricity throughout his body after receiving a dose of 16 micrograms/kg. of LSD.

Physiological effects were generally most intense at the higher dose levels, so that a certain degree of physiological distress occurred in many subjects at 8 and 16 micrograms/kg. These effects included hypertension, occasional vomiting and hyperventilation. A detailed description of physiological and biochemical effects will be the subject of a later report.

The time between drug ingestion and the first reported symptoms was progressively shorter with increasing dosage. At 1 microgram/kg. a half-hour or more often elapsed. At 16 micrograms/kg. effects were often noted within one to five minutes following ingestion. At this dose, hallucinations and thought disorder developed within five to ten minutes. The height of the psychological and physiological effects was generally within one or two hours post-ingestion at the lower doses. At the top doses maximal effects generally appeared within 45 minutes and lasted until about three hours post-ingestion.

The duration of effects increased with dosage. At 1 microgram/kg. most signs and symptoms usually were gone within six to eight hours. At 16 micrograms/kg. hallucinations, and some thought disorder, persisted for up to 24 hours.

DISCUSSION

These studies demonstrate a direct relationship between the dosage of LSD-25 and the psychophysiological response. This relationship is found mainly in certain aspects of thought processes, and perception, which we have described, as well as in autonomic effects and in the onset and duration of effects following ingestion.

These effects, which occur with little qualitative variation between subjects, may be considered, in a sense, the fundamental effects of LSD.[4] They must be distinguished from other effects, which vary considerably from one subject to the next and which are less a function of dosage. Examples of this latter group include paranoid and panic reactions, and psychotic depressions. Such effects appear to be determined more by personal predisposition than by dosage of LSD, provided an effective dose is given (3). We attribute our success in rating subjects as to dosage partly to our having made this distinction. If we had failed to make it we might have been misled by the more colorful side of the LSD reaction associated with the personality of the subjects taking the drug.

Another source of error which was avoided is the attempt to judge the effects of varying doses simply by comparing different subjects with one another. In our studies, each subject was compared primarily with himself at

[4] We use the term *fundamental* in a qualified sense, referring to the simpler psychophysiological effects of LSD which are routinely observed by clinical methods.

the different doses, and only secondarily with the others. Even with respect to the fundamental effects of LSD there was considerable variation between subjects in the severity of their reactions to a particular dose. On any single day it would have been very difficult for us to judge doses accurately.

Several questions remain. Could one have successfully rated subjects as to dose level by following changes in only one area, such as hallucinations, or, even more simply, by the time of onset and duration of effects? We did not attempt to do so, but our impression is that it might have been possible, although more difficult, to make judgments on the basis of such limited information.

Related to this is another question: Would not an objective, standardized psychological test have been in order as an index to the severity of the drug response? This would indeed have been a most valuable aid if it could have been done. Unfortunately, we have no test which is entirely dependable for this purpose. But even if there were one, it is doubtful that it could have been used, since, with large doses of LSD, very few subjects will cooperate with any kind of formalized testing.

We will consider one final question of another sort. In view of the large doses employed, one may wonder about selection of subjects for study and their management during drug reactions. In approaching this question, one should mention that we have seen reactions far more severe in some respects at doses of only 100 micrograms than any that occurred at the largest doses in these series. These included acute panic, highly systematized paranoid delusions, and profound depression. The problem of managing the subjects if such reactions occurred with any frequency in either of these series would have made it difficult, if not impossible, to perform these studies. In order to avoid or minimize the occurrence of severe reactions of this sort we attempted to choose the "best adjusted" subjects available and to provide a supportive environment for them, as indicated above.

SUMMARY AND CONCLUSIONS

Two related studies were carried out to determine the effects of varying dosage in the response of humans to LSD-25. Two series of dosage were employed, following a logarithmic scale. A latin square design was followed. In series #1 subjects received 1, 2 and 4 micrograms/kg. and in series #2, 4, 8 and 16 micrograms/kg. The largest single total dose was 1654 micrograms. LSD was administered a total of 36 times between the two studies, and the administration was double-blind with respect to dosage.

Three medical observers independently rated subjects on all drug days with respect to quality and severity of reactions. At the end of each series, all raters had the task of attempting to assign each subject to his actual place in the experimental design. In both studies all raters succeeded in this with complete accuracy. These results are highly significant statistically. On the basis of these studies we arrived at certain conclusions, as follows.

1. The severity of the psychophysiological effects of LSD in a given subject is proportional to the dosage, between 1 microgram/kg. and 16 micrograms/kg. This relationship holds for the "fundamental effects" of LSD, which we have described.

2. Certain other types of reaction to LSD, such as paranoid ideation, occur only occasionally and appear to be less a function of dosage than of personal predisposition.

3. Even with respect to the fundamental effects of LSD, there is considerable variability among subjects in the severity of their reactions to a particular dose.

4. Under carefully controlled conditions, systematic clinical observation can be not only a highly sensitive but also a reliable and valid method for the evaluation of the severity of drug reactions in human subjects.

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FUNCTIONAL LEVELS OF DISTURBANCE MANIFESTED BY BRAIN-DAMAGED (HEMIPLEGIC) PATIENTS AS REVEALED IN RORSCHACH RESPONSES[1]

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INTRODUCTION

This study is concerned with the investigation of functional changes resulting from brain damage which may be revealed by the analysis of Rorschach responses. Past studies which have used the Rorschach technique for exploring "brain damage" have had two basic concerns. One interest has been in the refinement of the Rorschach test as a diagnostic device for the detection of brain damage. The second and related focus has been the study of the effects of brain damage on personality and intellect by means of the Rorschach test. The work of Piotrowski (15), Harrower (7, 8) and Reitan (16, 17) is representative of the use of the Rorschach method for both purposes.

Although the Rorschach test has been widely used as an instrument for the study of brain damage, the results obtained have been less than satisfactory. Birch and Diller (2) have recently reviewed some of the contradictory findings and have extended the view of Klebanoff *et al.* (10) that to a considerable degree discrepant and contradictory findings are the result of the multiplicity of conditions that are masked by the use of a singular category, "brain-damaged." In part, some of the discrepancies are resolved when the overall category of "brain-damage" is more explicitly analyzed in terms of its anatomic and physiologic components (2).

A second possible source for discrepant results when the Rorschach test is used to study the functioning of brain-damaged patients resides in the use of traditional methods of scoring. It is possible that these methods, developed in largest part for the study of personality disturbance, may not represent the most adequate manner in which functional changes resulting from brain-damage may be revealed. One alternative has been the use of a "signs" test (14, 17). Another approach to the analysis of Rorschach responses in brain-injured individuals may be derived from the concept of functional levels, a view which has been emphasized by the recent re-issue of the selected papers of Hughlings Jackson (9). Some support for the potential fruitfulness of a levels analysis of Rorschach responses derives from the researches of Denny-Brown (5) and Bortner and Birch (3). These workers have shown that a re-analysis of well-known clinical phenomena in motor dysfunction and in visual-motor performance can be both productive and illuminating when a "levels" approach is utilized.

In the present investigation indications of dysfunction are sought by attempting to define the levels of psychological functioning that are discernible in Rorschach responses of patients who have sustained brain damage. These levels may be operationally defined by reference to the kinds of demand made on the patient by different aspects of the Rorschach test.

The first level of demand is one in which the patient is requested to view a complex stimulus and to give a verbal report on his perception. This level may be identified as a visual-verbal task, in contrast to Reitan's (16) use of these terms to describe the entire test process. Once a response to this task de-

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mand has been made a second level of demand is introduced by the test inquiry. In this phase of the examination the patient is asked to recall his percepts or is reminded of his percepts and is asked to specify in the stimulus the basis for his responses. This demand constitutes a request for analysis of the percept by the patient, and the level of function tapped may be designated as the perceptual-analytic level. A third level of demand is typified by the testing-of-limits. In this phase of the examination a percept not previously reported is suggested to the subject. He is then required to alter his perception to fit the verbal suggestion. This level may be designated as the verbal-visual level. Consideration of the Rorschach performances of brain-damaged and normal persons in terms of these operationally defined functional levels is the object of the present study.

SUBJECTS AND PROCEDURE

The subjects of this study were patients on the rehabilitation ward of a large municipal chronic disease hospital. Thirty-four subjects were used. Eighteen had left hemiplegia as a result of a cerebral vascular accident; all had damage to the right cerebral hemisphere. All were right-handed premorbidly. The sixteen orthopedically handicapped control subjects had no discernible central nervous system damage on examination. From an available population of approximately 400 left hemiplegics, subjects were selected who fitted the following criteria: 1) were chronic cases in whom hemiplegia had been present for at least seven months, 2) had no gross expressive or receptive language disturbance, 3) had no hemianopia or uncorrected visual or significant auditory defect. The non-brain-damaged subjects were selected to match the experimental group for age and sex. All patients came from a lower-class social background. Table 1 summarizes the patient characteristics.

Individual Rorschach testing was done with each subject. Standard procedures were

TABLE 1
Sex, Age and Number of Subjects

Group	Number of Subjects			Mean Age of Subjects		
	Total	Male	Female	Total	Male	Female
Left Hemiplegic	18	11	7	65.3 (46–81)	65.5 (46–81)	65.1 (57–72)
Non-hemiplegic	16	10	6	64.5 (50–79)	65.0 (50–79)	63.7 (54–77)

followed in the stimulus presentation and inquiry periods, with the addition of a special testing-of-limits procedure. To standardize the testing-of-limits procedure we compiled 21 popular responses from those reported by Klopfer (11) and Beck (1). In all cases a patient was asked to respond to verbal suggestions of all popular responses that had not previously been spontaneously reported by him. If the subject responded affirmatively, he was asked to delineate the suggested figure on the stimulus card and to locate its component parts. If the subject indicated that he was unable to see the figure, then the examiner outlined the specific area and requested the subject to look at the card again. If the subject was then able to see the figure he was asked to delineate it. If he was unable to see the figure even with the given hint, it was recorded as a rejected popular percept. Thus, a record was made of spontaneously seen popular figures, of those seen only when suggested, and of those popular percepts which were rejected by the subjects.

Since all the brain-injured patients were hemiplegic, the examiner was aware of the study group to which the given patient was assigned. All testing was done by the same examiner who, in order to reduce the possible bias introduced by his knowledge of the presence of neurological damage, used a strictly standardized testing procedure without special modifications for the individual patient. All verbalizations were fully recorded without interpretation. Standard scoring as outlined by Klopfer (11) and an analysis of the protocols in terms of the

concept of functional levels were carried out. Scoring was done by two individuals working in complete independence of each other, one of whom was the examiner and the other of whom had no knowledge of patient group assignment. Traditional Rorschach scoring and functional levels scoring were done for each protocol on separate occasions. Inter-scorer reliability was .87 for traditional scoring, .96 for the visual-verbal level scoring, and .80 for the perceptual-analytic level scoring.[4] In addition, three months after initial scoring the examiner re-scored all protocols in terms of functional levels without access to prior results and without information as to the individual whose protocol was being scored. This re-scoring resulted in 10 of 958 items being differently scored on the two occasions, an error of approximately one per cent.

The criteria used for the scoring of functional levels were as follows.

The visual-verbal level: The verbal responses made to the initial card presentation were scored in terms of the degree of precision and specificity of the reported percept. Scoring was to two categories, "well-defined" and "poorly-defined." If the subject indicated that he had seen a specific object, it was scored as "well-defined." Examples of responses so scored are: "a bat," "two sweethearts," "a skinned woodchuck nailed to a barn door." If the reported percept was unspecific, or had vague or confused elements, it was scored as "poorly-defined." Examples of "poorly-defined" responses are: "This appears to be a flower of some kind." "A water-color? Can't place it." "Some kind of animal." "This is a man or a bird." "Looks like something in the center but I can't make out what it is." "I see his two feet and head. I never seen these things. I don't know what they are." The adequacy of this level was scored solely upon the subject's clarity of statement in the visual-verbal period and was independent of whether it was well or poorly analyzed during the inquiry.[5] Subjective uncertainty, though present, was not scored as such. (For example, "Is it a bear?" was scored as "well-defined.") Thus, the scoring referred only to the level of perceptual clarity verbalized by the subject, even if the verbalization was couched as a question.

The perceptual-analytic level: The inquiry responses of the subject were scored as "well-analyzed" if the subject was able to identify clearly the parts and qualities of the blot that contributed to his perception independently of the value of the original perception itself.

A "poorly analyzed" score was assigned to a response when the subject was vague or unspecific, radically shifted the nature of the object or figure, or presented a distorted, disorganized or incoherent analysis. Examples of "poorly analyzed" responses are: 1) (Initial perception) "To me it looks like two animals laying side by side. Tiger or lion." (Inquiry) "Two lions or tigers. All fur." (S could not indicate the parts.) 2) (Initial perception) "Looks like a grasshopper." (Inquiry) "Looks like a crab—or a spider." 3) (Initial perception) "May be two people with their hands together." (Inquiry) "Maybe the head is here (top of blot) or here (bottom of blot). I can't tell."

The verbal-visual level: The scoring of the verbal-visual level was based on the subject's ability to make a perception which fitted the verbal suggestions presented by the examiner. Each suggested percept was scored as "accepted" or "rejected."

RESULTS

An analysis was made of the hemiplegic and non-hemiplegic subjects' responses scored in the traditional system. The data which revealed significant differences are presented in Tables 2, 3 and 4.

[4] The Kuder-Richardson formula #20 for dichotomous data as reformulated by Lorr (12) was used to calculate the reliability coefficients.

[5] Friedman (6) and Phillips *et al.* (14) have incorporated a similar differentiation within the framework of the traditional Rorschach scoring procedures.

It can be seen from Table 2 that the hemiplegic patients were significantly less productive than were the non-hemiplegic patients. This result is in agreement with expectation and iterates the finding that brain-damaged patients are less productive than are non-brain-damaged subjects.

Since the number of Rorschach responses made by the non-hemiplegic subjects is, on the average, almost twice as great as those made by the hemiplegic patients, it is not possible to engage in any direct comparisons of response characteristics (*e.g.*, location, determinants and content) without partialling the data with respect to differences in the numbers of responses. In part, this can be done by treating Rorschach scores as fractions of the response total. However, as Cronbach (4) has pointed out, this frequently used procedure involves serious methodologic weaknesses. Such difficulties are avoided if a modified median test is used. No significant differences were obtained between groups on standard Rorschach scores when differences in response total were taken into account through the use of a combined group median. However, when a cut-off criterion was established at the 50 per cent point, one difference between the groups was clearly demonstrated. In Table 3 the number of subjects in each group who gave 50 per cent or more W (whole responses) are contrasted with those who gave less than 50 per cent W responses. As may be seen, a significantly greater proportion of subjects in the hemiplegic group exhibited a preponderant tendency to make W responses. No other response category exhibited inter-group differences of statistical significance.

The groups also differed significantly from one another in terms of the number of subjects who rejected one or more cards. As may be seen in Table 4, more hemiplegic patients rejected one or more cards than did the non-hemiplegic patients. This finding further emphasizes the productive incapacity of the brain-damaged patients in contrast

TABLE 2

Total Number of Responses Produced by the Two Groups of Subjects

	Hemiplegic Subjects (N = 18)	Non-hemiplegic Subjects (N = 16)
Mean	10.4	18.2
S.D.	5.2	8.4

$t = 3.29$; $p < .01$.

TABLE 3

Number of Subjects in Each Group Who Gave More or Less than 50% W Responses

	Hemiplegic Subjects	Non-hemiplegic Subjects
>50% W responses	9	2
<50% W responses	9	14

Chi-square = 3.86; $p < .05$.

TABLE 4

Number of Subjects in Each Group Who Rejected One or More Rorschach Cards

	Hemiplegic Subjects	Non-hemiplegic Subjects
No. of subjects who rejected one or more cards	12	2
No. of subjects rejecting no card	6	14

Chi-square = 8.15; $p < .01$.

to the greater productivity of the non-brain-damaged patients.

Tables 5, 6 and 7 present the results obtained when the responses were analyzed in terms of functional levels of demand. As may be seen from Table 5,[6] no significant difference in the quality of response was manifested between the hemiplegic and non-hemiplegic groups at the visual-verbal level of functioning. Thus, although the brain-damaged patients perceived fewer things, what was perceived was well-reported.

At the perceptual-analytic level it is clear (Table 6) that a significantly greater number of non-hemiplegic subjects exhibited the

[6] The 60% point was selected as the cut-off point in Tables 5, 6 and 7 because a large gap in scores was discerned to exist above and below this scoring interval on data inspection.

TABLE 5

Number of Subjects in Each Group Who Gave More or Less Than 60% Well-Defined Responses on the Visual-Verbal Level

	Hemiplegic Subjects	Non-hemiplegic Subjects
>60% well-defined responses	12	12
<60% well-defined responses	6	4

Chi-square: N. S.

TABLE 6

Number of Subjects in Each Group Who Gave More or Less Than 60% Well-Analyzed Responses on the Perceptual-Analytic Level

	Hemiplegic Subjects	Non-hemiplegic Subjects
>60% well-analyzed responses	6	12
<60% well-analyzed responses	12	4

Chi-square = 4.35; $p < .05$.

TABLE 7

Number of Subjects in Each Group Who Rejected More or Less Than 60% of the Suggested Popular Responses

	Hemiplegic Subjects	Non-hemiplegic Subjects
>60% rejected	10	7
<60% rejected	8	9

Chi-square: N. S.

capacity to analyze their own spontaneously reported percepts. Brain damge, then, led to a loss in the ability to analyze perceptions into component and contributive parts.

In Table 7 it may be seen that no significant difference was manifested between the groups in the capacity to make new perceptual organizations on the basis of verbal suggestions.

DISCUSSION

In considering the nature of perceptual impairment which may occur as the consequence of damage to the brain, it is necessary to make a distinction between the capacity to recognize and the ability to analyze complex patterns of visual stimulation. Although it is commonly reported that brain injury results in impaired perception, the data of the present study provide support only for the view that perceptual-analytic capacity, rather than perception in general, is affected by the nervous system damage. This conclusion is supported both by the evidence of the perceptual-analytic treatment of responses and by the preponderance of whole responses that were characteristic of the brain-damaged group.

The distinction between perception recognition and perceptual analysis is customarily blurred by the traditional Rorschach procedure of combining responses to the initial and the inquiry periods on the assumption that the inquiry is merely an elaboration of the initial association period. Klopfer states, "The inquiry period can thus be seen as a process of attempting to ascertain the implicit thinking of the subject during the prior, or performance proper, phase" (11, p. 11). However, as the data of this experiment indicate, a distinction of importance can be made between the subject's ability to describe his perceptions and his ability to analyze them.

A similar distinction was made between perceptual functioning and perceptual-motor integrative functioning by Bortner and Birch (3) when they examined the defective block design performance of brain-damaged subjects. Their data led them to the conclusion that most of the faulty performances of the brain-injured patients they tested were related not to a perceptual deficit but rather to a perceptual analytic or an apractic difficulty. They conceive of the apractic deficit in this instance to "involve an inability to translate a percept into an appropriately organized pattern of acts" (3, p. 52). The work of Bortner and Birch and of this experiment leads to the conclusion that it is necessary to differentiate associative perception or recognition from a perceptual-motor level and from a perceptual-analytic level of functioning.

It may be useful at this point to attempt

o delineate further the differences between he visual-verbal and the perceptual-nalytic levels. Perception, relative to Ror-schach performance, involves sensory input nd the relating of this input to the symbolic rocess. The objective aspects of the figure-ground qualities, of color and of shading as hey exist on the blot are organized separately nd in combinations in terms of the ideational ystem possessed by the subject. The prod-ct that results when sensory input is haped by the symbolic process is experi-nced as single, total "gestalten." The sub-ject is then expected to report the individual percepts to the examiner. This has been termed the visual-verbal level.

The perceptual-analytic is a more complex level. On this level, the subject is called upon to isolate one feature and then another of the blot to indicate which features determined his total percept. The figure-ground aspects constituting a specific form must be distingusihed from color, and color from shading, etc., while at the same time the subject must refer each isolated feature to his total percepts. Further, and of more importance, the process of analyzing the percepts not only calls for an active review of possible blot features that may be involved but also provokes the creation of new thoughts about and reactions to the original percept. Clearly then, the inquiry session is not merely a time for the verbalizing of previously unstated perceptual qualities, but is rather a new and more complex level of involvement. The significantly increased failure in analytic ability of the neurologically damaged subjects used in this study reveals, then, that it was not at the simpler level of perceiving that failure most markedly occurred, but instead at the more complex level of integrating perceptual, analytic and synthetic properties. Thus, the failure on the analytic level reflected the inability to differentiate and coordinate what is perceived with what is thought and with what is said

Another finding of this study is that the brain-injured patients had not lost the ability to utilize extrinsic verbal suggestion for the facilitation of "popular" visual perceptions. In almost all instances, the subject, once able to perceive the verbally suggested item, had no difficulty in determining the main elements which constituted the percept. The key to the nature of the verbal-visual level is that these are language-determined percepts, which are spontaneously deficient, but not unavailable when extrinsically facilitated by language.

Denny-Brown's recent consideration of apraxia (5) is also of interest. Using clinical evidence derived from his examination of brain-injured patients with disturbed motor functioning, Denny-Brown has come to the conclusion that there are two fundamentally different kinds of processes involved in the impaired motor pattern of apractic performances. One he terms ideational apraxia, in which faulty purposive movements are related to losses in the ideational process. The other he terms kinetic apraxia, which is defined as being an unwilled, purposeless motor response to direct stimulation. Denny-Brown states, "All types of kinetic apraxia are to be sharply distinguished from the motor disorders traceable to absence or perseveration of the propositional aspect of the perception" (5, p. 4). Thus, an important distinction is made between disturbed motor performance based on defective sensory-motor relations and disturbed motor performance based on defective conceptual-motor integrations.

The studies of apraxia focused on the variety of disrupted functional relationships that underlie disturbed motor patterns. Our analysis of the Rorschach test provides complementary evidence for examining levels of afferent organization. It therefore provides further evidence on perceptual-conceptual phenomena which may be of use in developing a more complete view of the interacting processes that result in different

kinds of disturbed behavior in brain-damaged persons.

SUMMARY

The Rorschach test was administered to eighteen brain-injured (left hemiplegic) patients in their sixties and to sixteen equally old non-neurologically damaged (orthopedically handicapped) controls. The responses of the subjects were: 1) scored using the traditional scoring method outlined by Klopfer, and 2) re-scored in terms of the differing functional demands posed by the initial, inquiry and testing-of-limits periods as the scoring categories.

The analysis of the traditional Rorschach scores confirmed previous conclusions regarding some consequences of brain injury. The brain-injured subjects expressed impotence in: 1) lowered productivity, 2) increased acceptance of the stimulus as a whole, with lessened ability to re-organize the stimulus in terms of its parts.

When the Rorschach responses were analyzed by functional levels, three major conclusions emerged. As a consequence of the brain injury sustained by the left hemiplegic patients used in this study: 1) there was no evidence of impaired whole perception, 2) there was clear evidence of impaired ability to analyze visual perceptions, and 3) there was no evidence of impaired ability to alter visual perception on the basis of verbal suggestion.

The approach and the findings of this study were related to other work which has as its aim the elaboration of various levels of psychological functioning. It was suggested that greater attention to such a functional approach is required in the study of the consequences of brain injury.

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THE VALIDITY OF THE HEWSON RATIOS: A CRITIQUE OF WOLFF'S STUDY

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The August, 1960, issue of this Journal reported an interesting study (5) on the effectiveness of the Hewson Ratios (2) in identifying patients with various types of cerebral pathology. Although there have been several other studies on this topic (1, 3, 4), Wolff's is the first in which the Ratios are derived from scores obtained on the Wechsler Adult Intelligence Scale (WAIS), instead of from the older Wechsler-Bellevue Intelligence Scale, Form I (W-B I). More importantly, it is the only study that seems to offer convincing evidence that the Hewson Ratios are sensitive to the intellectual consequences of brain pathology, while being unaffected by variations in the ages and intellectual levels of the patients studied. Before the body of clinicians decide to start using this method as an aid in differential diagnosis, however, it would be well to examine Wolff's findings thoroughly.

The purpose of this paper will be to focus on what seem to be crucial weaknesses in Wolff's research. Specifically, I intend to show that the method employed in selecting patients for the experimental and control groups is subject to serious criticism, that the procedure used in assessing the effects of age and IQ on the results is wholly inadequate, and that there remains to date no scientifically sound basis for the belief that this diagnostic technique is in any way sensitive to the intellectual *sequelae* of cerebral pathology.

Since Wolff's study is largely an attempt to overcome objections to the Hewson Ratios raised by the findings of McKeever and Gerstein (3), a brief summary of the latters' work provides a helpful background against which to evaluate Wolff's investigation. This study attempted to differentiate a group of 52 brain pathology cases from 50 schizophrenic cases by means of the Hewson Ratios. Although Wolff questions the adequacy of the criteria for selecting these patients, they were chosen from a patient pool of over 600 cases, and their diagnoses were well documented and mutually exclusive. The matching of these diagnostic groups on both age and IQ was sufficiently refined that, even when they were divided into subgroups according to two levels of intelligence, there were still no differences in the ages of any of the four groups. This last point is important because it contrasts sharply with the procedure employed by Wolff.

The results of this study showed the Hewson Ratios to be incapable of differentiating the diagnostic groups. Further, it was shown that the likelihood of a patient's obtaining a score indicative of cerebral pathology was an increasing function of both increasing age and decreasing intelligence. It follows from these findings that any study which fails to control for these variables will produce results favorable or unfavorable to the Hewson technique, strictly in accordance with the extent to which the age and IQ distributions of the groups are weighted in the direction of the organic groups' being older and less intelligent. Thus Gutman's (1) results, the only positive ones ever published aside from Wolff's, are highly suspect, since the data presented on age and IQ suggest that her "brain-damaged" group was considerably older and less intelligent than the controls.

Although two of the three published validity studies on the Hewson Ratios are unequivocally negative (3, 4), and the third (1) of very doubtful validity, Wolff writes

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as though there had never been any question but that the Hewson technique was an established, valid test of cerebral pathology. In the introduction to his paper he says, "... if it could be shown that Hewson's Ratios could be applied to the WAIS they could continue to serve a useful purpose." Surely the weight of research evidence suggests that if they "continue" to serve the same purpose as when applied to the W-B I, it could be only as a relatively poor predictor of patients' ages and IQs. This same overwillingness to think well of the ratios is apparent also in Wolff's discussion of the results of the "ordinary J-Scores" as well as in the summarization of the article. These points call for criticism because they paint an unjustifiably positive picture of the scientific status of the ratios prior to Wolff's study, and tend to be misleading for those readers who are unacquainted with the first-hand reports of previous research.

Now let us examine the actual research aspects of Wolff's article. Despite his knowledge of the demonstrated importance of age and IQ variables in producing scores indicative of cerebral pathology, he makes no attempt at pre-experimental control of these factors. As a result, the experimental (brain pathology) group turns out to be seven years older and sixteen IQ points less intelligent than the main control group. The additional arthritic controls are the same age as the experimental group and slightly brighter. It is evident that these differences correspond roughly to the relative frequencies of scores indicating cerebral pathology in the three groups, *i.e.*, by any of the three types of scores Wolff uses (ordinary J-Score, "age-corrected" J-Score, or W-Score), the oldest-least-intelligent (brain pathology) group has the highest percentage of organic scorers, the youngest-brightest (main controls) group has fewest, and the arthritic control group falls between them. Therefore, the age and IQ characteristics of the groups are precisely those which would be expected to produce results favorable to the Hewson technique, solely as an artifact and without any necessity of a relationship between organic scores and organic brain pathology.

Wolff can argue, of course, that although age and IQ were not controlled, his analysis indicates no systematic influence of IQ on any of the three types of scores, and that while age was found to contaminate the ordinary J-Scores in a manner consistent with the results of the McKeever-Gerstein study, the use of the "age-corrected" J-Scores overcame this difficulty. Had Wolff used matched groups, this argument would be valid, but in actuality the effects of age and IQ as assessed by Wolff's method are indeterminate.

The method Wolff uses to study the effects of age and IQ attempts to show that the percentages of patients within a diagnostic category who earn organic J-Scores are similar for young as contrasted to old patients on the one hand, and for more as contrasted to less intelligent patients on the other hand. The basic failing of this procedure is that it offers no assurance that the results do not reflect an interaction of the effects of age and IQ, for it makes the unwarranted assumption that the effects of the variable not being analyzed are constant for the two conditions of the one under analysis.

As a concrete example, Table 3 of Wolff's article shows that for the "age-corrected" J-Scores, 61 per cent of the young and 63 per cent of the old patients score "organically." Since a two per cent difference is not significant, Wolff concludes that: 1) increasing age does not have a tendency to produce increasing percentages of organic J-Scores, and 2) the Ratios are sensitive to the effects of brain pathology, *per se*, in nearly two-thirds of patients with brain pathology. Now since both old age and low intellectual level are associated with increasing likelihood of organic J-Scores (3), an important question is omitted in the foregoing analysis, namely, what are the mean IQs of these young and old subgroups?

Suppose, for example, that the younger group is actually considerably less intelligent than the older group, a point on which Wolff offers no direct information. A moment's reflection makes clear that if this is so, both of Wolff's conclusions are vulnerable and an alternative set of conclusions becomes tenable: 1) the reason nearly two-thirds of the young patients score "organically" is not that they have brain pathology, but that their low IQs produce their organic J-Scores, and 2) the effects of age and IQ are confounded in these comparisons in such a way that the effects of each are masked by the other. The same type of criticism applies equally to the IQ comparisons Wolff makes, *i.e.*, what are the mean ages of the high and low IQ subjects within each diagnostic group? Thus, using Wolff's method of assessing the effects of age and IQ, seemingly positive results can be produced as an artifact of age and IQ correlations with "organic performance," provided age and IQ are inversely related within the main diagnostic groups, but the absolute differences between diagnostic groups tend strongly in the direction of the experimental group's being older and less intelligent.

Is there any evidence to suggest that these conditions really obtained in Wolff's study? With respect to the direction of absolute differences, Table 1 of the article demonstrates beyond any doubt that the experimental group was, in fact, much older and less intelligent than were the main controls. Although insufficient information is presented to allow the definite conclusion that age and IQ are inversely related within the main diagnostic groups, the "male-female" breakdown of the data in Table 1 certainly suggests that there is at least a tendency in this direction. For example, in both the experimental and main control groups, it appears that the youngest patients (predominantly women) are also less intelligent than the older patients.

Does Wolff's study support the validity of Hewson's Ratios as applied to the WAIS? Had Wolff used matched groups, or, lacking that, had he computed and presented the mean ages of the high and low IQ patients and the mean IQs of the young and old patients within the diagnostic groups, he would have provided evidence one way or the other. In the absence of these essentials, there seems to be at least as much reason to think Wolff's results are spurious as there is to accept them at face value.

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THE VALIDITY OF THE HEWSON RATIOS: A REPLY TO McKEEVER'S CRITIQUE

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McKeever's interesting critique on the validity of the Hewson Ratios, reported in this issue of JNMD (2), contains two major criticisms of Wolff's study (7): 1) that the selection technique of the subjects for the experimental and control groups was inefficient, and 2) that the method used to assess the effects of age and intelligence on the ratios was inadequate. McKeever therefore concludes that "there remains to date no scientifically sound basis for the belief that this diagnostic technique is in any way sensitive to the intellectual sequelae of cerebral pathology."

The writer is grateful to McKeever for exposing possible methodological shortcomings of his study, but at the same time feels that the critique contains several misapprehensions which should be clarified briefly before the two major points at issue are discussed.

Wolff's study (7) is certainly not a direct attempt to overcome objections to the Hewson Ratios raised by McKeever and Gerstein's paper (4). On the contrary, Wolff commenced his study in 1957, prior to the publication of the latter's article in 1958, and from the very beginning he also postulated that the original Hewson Ratios are contaminated by an age factor when used with the Wechsler-Bellevue weighted scores or the ordinary WAIS scaled scores. This point was made very clearly in the introduction to his paper (7, pp. 99), and the findings strongly support this view as well as the results of McKeever and Gerstein's study concerning age, although the effects due to intelligence were found to be statistically insignificant. It is surprising that McKeever in his critique apparently fails to take cognizance of the fact that Wolff's study largely supports his and Gerstein's investigation. It must, however, be emphasized that McKeever and Gerstein used the Wechsler-Bellevue weighted scores; therefore their results can be compared directly only to those of Wolff based on the *ordinary* WAIS scaled scores, but not to the ones utilizing the *age-corrected* WAIS scaled scores. The fact that Wolff found the Hewson Ratios to be useful when they are based on *age-corrected* scores surely in no way minimizes the value of McKeever and Gerstein's investigation.

Wolff (7) discussed *four* previous validation studies, including that of McKeever and Gerstein, and he found two to support and two to reject the validity of the Hewson Ratios. McKeever selects only *three* of these studies in his critique, his own and those of Gutman (1) and Wheeler and Wilkins (6), and omits any reference to that of Nicholas and Thetford (5). While his omission may be justified insofar that he uses the term "published," as the last-mentioned study was reported at a meeting of the Eastern Psychological Association but not published, for the sake of accuracy he should at least cite this study, especially as it is referred to in Wolff's article. Furthermore, McKeever states that the results of two out of the three investigations selected by him are unequivocally negative, but it is submitted that this is open to controversy. In their study Wheeler and Wilkins (6) did not include any patients with known organic brain pathology; instead, they used 84 neurotic, 113 functional psychotic and 39 normal subjects, and their results led them to the conclusion that the Hewson Ratios are not sufficiently valid for the differential

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diagnosis of neurosis, functional psychosis and normalcy. In addition, they hypothesized that the ratios may also not be sufficiently valid for the diagnosis of cerebral pathology, because a relatively large proportion of their subjects obtained organic Hewson scores (*i.e.*, 21 per cent of normals, 26 per cent of neurotics and 44 per cent of psychotics). The results of their study are considered unequivocally negative by McKeever, as are the results of his own investigation. However, using Wheeler and Wilkins's published data, Wolff dichotomized their results into organic-nonorganic Hewson categories and found that 66 per cent of their subjects were correctly classified by the Hewson Ratios; this is not dissimilar to the findings of 71 and 68 per cent respectively of correct Hewson diagnoses obtained in Gutman's (1) and Nicholas and Thetford's (5) studies. This modification of Wheeler and Wilkins's results, in addition to the absence of subjects with cerebral pathology, renders their data equivocal rather than unequivocal. Wolff already discussed this point in his previous paper, although McKeever does not refer to it in his critique.

McKeever also criticizes Wolff for overwillingness to think well of the ratios. Actually, however, both McKeever and Wolff are critical of the original Hewson Ratios, but whereas Wolff uses a constructive approach in investigating the validity of the ratios by applying WAIS scores corrected for age (after showing contamination by age when not corrected in this fashion), McKeever apparently takes a negativistic attitude following his and Gerstein's study, which showed both age and intelligence to affect the original ratios. Arguing by analogy from these findings, McKeever is of the opinion that the age-corrected ratios are not much better than the original ones regarding age and intelligence factors. Such an analogy from the original to the age-corrected ratios is really not quite permissible in the absence of additional data to refute Wolff's conclusions.

McKeever's first major criticism refers to Wolff's lack of pre-experimental control of age and I.Q. variables. In their study McKeever and Gerstein matched their experimental and control subjects for both age and intelligence by selecting them from a patient pool of over 600 cases. One of the best techniques for assessing the effects due to age and I.Q. is by means of matched groups, like those of McKeever and Gerstein. It does not follow, however, that the failure to use matched groups is necessarily methodologically unacceptable, provided the data are analyzed by appropriate statistical procedures. Practical considerations prevented Wolff from using matched groups, especially as his approach differed from that of McKeever and Gerstein. Nearly all of Wolff's subjects, excepting the arthritics, were patients with some neurological or pseudo-neurological symptoms, and had been referred by the neurologist for psychological evaluation to assist in the diagnosis. His subjects, accordingly, could not be matched for age and I.Q. in advance, especially as the definitive diagnosis was not known at the time of the psychological examination. After the termination of his investigation, Wolff could have selected only those of his experimental and control subjects who could be matched for age and intelligence, but this in turn would have reduced the number of available subjects unnecessarily without adding to the value of his study (assuming, of course, the use of appropriate statistical methods). In view of the practical considerations which influenced Wolff's choice of subjects, McKeever's objection to the selection procedure can be more suitably considered in the discussion of his criticism of the statistical analysis of the results, because it is not the method of selection as such but the choice of an adequate statistical treatment of the data appropriate to the selection procedure employed which is at issue here.

At this point it might be suggested that, although they carefully matched their sub-

jects for age and intelligence, McKeever and Gerstein apparently paid less attention to their diagnostic criteria for the selection of their experimental and control subjects, which was made on the basis of hospital records. They offer no evidence 1) on what basis the clinical diagnoses were made, and 2) whether or not all their subjects underwent similar diagnostic screening procedures. One may assume that, as a matter of routine clinical practice, all of their experimental organic subjects probably had been given some objective clinical or laboratory test to confirm the diagnosis of cerebral pathology. On the other hand, lacking evidence to the contrary, the assumption that all the schizophrenic controls were screened neurologically to exclude cerebral pathology cannot be made. Had they used normal subjects or non-psychiatric patients as controls this point would not be very significant, but in the case of the psychotic subjects it is possible that some of the control subjects actually had organic brain pathology as well, masked by or in addition to the schizophrenic reaction pattern (although McKeever and Gerstein naturally did not include cases with a *known* mixed diagnosis of schizophrenia and cerebral pathology). Their procedure contrasts sharply with Wolff's rigorous diagnostic criteria, which required every organic patient to have had at least two independent and objective laboratory tests, surgery or autopsy to confirm the presence of cerebral pathology; similarly every main control subject (except the additional arthritic group) had been given at least two independent and objective laboratory tests, with negative results. Forty-seven neurological patients did not meet these strict diagnostic criteria and were therefore not included in Wolff's investigation. The additional arthritic control group did not have this intensive neurological screening procedure and was thus analyzed separately.

TABLE 1

Product-Moment Correlations between Age and WAIS Full Scale I.Q.

Subjects	N	r
1. All Subjects	150	−.096*
2. Experimental Organics	65	−.009*
3. Control Non-organics	58	.079*
4. Additional Control Arthritics	22	−.023*
5. All *Ss* with Organic Age-corrected Hewson J-Scores	62	−.197*
6. All *Ss* with Non-Organic (*i.e.* Normal and Functional) Age-corrected Hewson J-Scores	88	.059*
7. Difference between r_5 and r_6	150	$t = 1.55$*

* Not significant.

McKeever's chief and most important criticism is that Wolff's statistical analysis of his data to assess the effects of age and intelligence variables is inadequate. Wolff, applying the age-corrected WAIS scaled scores to the Hewson Ratios, first compared the results for young and old subjects, and then those for low and high intelligence subjects, and demonstrated that the effects of age alone or intelligence alone were statistically insignificant for the age-corrected ratios. The crucial point of McKeever's critique is that this analysis does not decisively assure that there is no *interaction* of the effects of age and I.Q., and that it is unwarranted to make the assumption that the effects of the variable not being analyzed are constant for the two conditions under analysis. McKeever, on the basis of his and Gerstein's results, postulates the hypothesis that the likelihood of obtaining a Hewson score indicative of cerebral pathology is an increasing function of both increasing age and decreasing intelligence. He suggests (3) that the most direct and relevant test of his hypothesis is to compute the correlations between age and I.Q. for each of Wolff's main groups. His hypothesis demands that age and I.Q. be significantly and negatively correlated, and therefore "if the correlations are positive or not significant, then the Hewson Ratios are not con-

taminated by age and I.Q. factors, and we have what would appear to be a useful test of cerebral pathology in the age-corrected J-Score" (3). In order to test McKeever's hypothesis, Wolff's data were re-analyzed and product-moment correlations between age and I.Q. computed, as shown in Table 1. No significant correlations between age and intelligence for all subjects, the experimental group and the two control groups[2] were found. The crucial test of McKeever's hypothesis is, however, the correlation for the group of subjects all of whom obtained organic age-corrected Hewson J-Scores (Table 1), and this is found to be statistically not significant ($r = -.197$). Furthermore, the difference in the correlations between subjects with organic and those with non-organic age-corrected Hewson J-Scores yields $t = 1.55$, which also fails to reach statistical significance. Wolff's results, therefore, do *not* support McKeever's hypothesis, but confirm the previous conclusion, that the Hewson Ratios when based on age-corrected scaled scores serve a useful purpose as a psychometric index of cerebral pathology. It should, however, be indicated that, as the correlation between age and I.Q. for the cases with organic Hewson J-Scores is negative, and larger than that for subjects with a non-organic J-Score, the possibility may exist that there is a slight but insignificant trend in accordance with McKeever's hypothesis, although it has little effect on the outcome of the ratios.

In his critique, McKeever suggests an alternative method of testing his hypothesis, namely, to compute and compare the mean ages of the low and high I.Q. patients and the mean I.Q.s of the young and old patients *within* the diagnostic groups. The hypothesis demands that *within* each diagnostic group the high intelligence subgroup should be older than the low intelligence subgroup, and the young subgroup should be less intelligent than the old subgroup. It will be shown that this alternative comparison technique is not quite so decisive and efficient as the previous correlation method, but it will be discussed, because it demon-

TABLE 2

Mean Ages, Mean WAIS Full Scale I.Q.s, and Proportion of Organic Age-Corrected Hewson J-Scores

Subjects	Mean Age Years	Mean I.Q.	Organic J-Scores	
			Incidence	Percentage
A. All Organics (N = 67)	42.5	87	43	64%
1. Young, Low Intell. (N = 25)	27.6	81	15	60%
2. Young, High Intell. (N = 9)	33.1	106	6	67%
3. Old, Low Intell. (N = 25)	56.6	79	18	72%
4. Old, High Intell. (N = 8)	56.1	108	4	50%
B. All Non-organics (N = 83)	37.5	100	19	23%
1. Young, Low Intell. (N = 28)	30.5	89	8	29%
2. Young, High Intell. (N = 27)	28.7	111	5	19%
3. Old, Low Intell. (N = 15)	54.2	89	4	27%
4. Old, High Intell. (N = 13)	51.6	112	2	15%
C. 1. All Young *Ss* (N = 89)	29.4	95	34	38%
2. All Old *Ss* (N = 61)	54.9	92	28	46%
3. All Low Intell. *Ss* (N = 93)	40.5	84	45	48%
4. All High Intell. *Ss* (N = 57)	38.5	110	17	30%
D. 1. All *Ss* with Organic Age-corrected Hewson J-Scores (N = 62)	42.0	87	62	100%
2. All *Ss* with Non-organic Age-corrected Hewson J-Scores (N = 88)	38.0	99	0	0%

Age: Young = 16–44 years; Old = 45–69 years.
I.Q.: Low = 52–99; High = 100–137.

[2] Wolff had 150 subjects, of whom 67 had cerebral pathology (*i.e.*, 65 experimental organics and two arthritics with cerebrovascular accidents), and 83 had no organic brain pathology (*i.e.*, 58 control non-organics, 22 additional control arthritics, and three extra control arthritics with manual motor deficits).

strates to some extent the basis for McKeever's criticism, and at the same time gives some support to his hypothesis. In line with McKeever's first contention, the differences in mean age of 5 years and mean I.Q. of 13 points (Table 2, A and B) between all patients with cerebral pathology and all controls are significant at the .05 and .01 levels, respectively. However, the crucial but equivocal results for the validity of the Hewson Ratios are those for all subjects with organic and with non-organic J-Scores (Table 2, D), where the difference in mean I.Q. is significant at the .01 level, whereas that for mean age is not significant. There are no significant differences in mean I.Q. between all young and all old subjects, and in mean age between all low and all high intelligence subjects (Table 2, C). Furthermore, contrary to McKeever's suggestion, the differences in mean I.Q. between, respectively, the various young and old subgroups *within* each diagnostic category, and the differences in mean age between, respectively, the various low and high intelligence subgroups *within* each diagnostic group, are also insignificant (Table 2, A and B, 1–4). The only exception is the difference in mean age between the low and high intelligence young organic subgroups (Table 2, A, 1–2), which reaches the .05 level. This is the only result of some importance which is consistent with McKeever's main argument, because in this specific case it is indeterminate whether the insignificant difference of seven per cent in the proportion of organic J-Scores between these two subgroups may be credited to the efficiency of the ratios, or, according to McKeever's hypothesis, accounted for by the interaction of contaminating factors due to increasing age and decreasing intelligence. (That is, the young, high intelligence organic subgroup is both older and more intelligent than the young, low intelligence organic subgroup, instead of just being more intelligent, and therefore the lack of difference in the organic J-Scores may be due to the counter-action of increased age and increased I.Q.). It is therefore clear that the previous correlation method is the more efficient and decisive technique, because it directly assesses the interaction of age and intelligence and presents an unequivocal answer to McKeever's hypothesis.

The evidence given in this reply to McKeever's critique clearly supports Wolff's original statement that the Hewson Ratios, when based on the age-corrected WAIS scaled scores, appear to be valid and can serve a useful purpose in the differential diagnosis of cerebral pathology, provided the cautions and limitations discussed in his study are observed. The data presented in this paper also refute McKeever's hypothesis. Nevertheless, this hypothesis is sufficiently important—in spite of the present negative evidence—to deserve consideration in future studies, especially as increasing age and decreasing intelligence may perhaps play a minor, but not statistically significant, role on the frequency of occurrence of organic age-corrected Hewson J-Scores. In any case, another validation study of the age-corrected Hewson Ratios is required to confirm Wolff's positive results and to recheck McKeever's hypothesis.

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A COMPARATIVE STUDY OF SELECTED TRANQUILIZERS IN THE TREATMENT OF PSYCHIATRIC PATIENTS

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Today the use of tranquilizers is generally accepted as standard psychiatric practice whenever anxiety and tension are prominent among the symptoms. Although there is some evidence that these drugs do more than just reduce intense emotionality, specific indications for their use are still not clearly defined.

This paper is a report of a study of the relative effectiveness of four tranquilizers in the treatment of hospitalized psychiatric patients. Meprobamate (Equanil) was compared with prochlorperazine (Compazine) in the treatment of mild psychiatric disturbances, and chlorpromazine (Thorazine) with perphenazine (Trilafon) in the treatment of more severe disturbances. The purpose of the study was to determine the nature and extent of changes following treatment with these tranquilizers. As the same set of variables was used in evaluating patients before and after treatment, it was possible to define improvement in terms of specific changes. It was hypothesized that patients in the comparable drug groups would show no significant difference in amount or kind of improvement following treatment.

PROCEDURE AND DESCRIPTION OF SAMPLE

At the time the study was begun prochlorperazine and perphenazine had not been used to any extent at this hospital. Where treatment with tranquilizers seemed indicated, meprobamate was prescribed for patients who gave the clinical impression of being mildly disturbed, and chlorpromazine for those who appeared more severely disturbed. For purposes of comparison it was decided that, of the newly admitted patients who were considered suitable for tranquilizing medication, those showing mild symptomatology would be assigned to meprobamate and prochlorperazine on an alternating basis, and those showing severe symptomatology would be assigned to chlorpromazine and perphenazine on an alternating basis.[2] This was the only change in the treatment program. All subjects went through the regular admission procedure, and all aspects of hospitalization remained similar from one patient to the next with the exception of the treatment variable under study. The length of time that the patient continued on medication was determined by the ward physician. All medicine was given orally, except in one instance where the patient was given medication intramuscularly for a brief period. Dosage was adjusted to what appeared to be the needs of the patient. Maximum daily dosage ranged from 800 to 1200 mg. (median, 1200 mg.) for patients in the meprobamate group, from 20 to 150 mg. (median, 40 mg.) for the prochlorperazine group, from 50 to 1600 mg. (median, 600 mg.) for the chlorpromazine group, and from 12 to 208 mg. (median, 32 mg.) for the perphenazine group. One patient on meprobamate also received multi-vitamins and one on perphenazine received Gelusil. Toward the end of the first four weeks on tranquilizers two perphenazine patients were started on Marsilid, three perphenazine and one chlorpromazine patient were started on Cogentin, and one meprobamate patient received Benadryl. None of the patients received psychotherapy or any other specific

[1] Veterans Administration Hospital, St. Cloud, Minnesota. This study was conducted in connection with the Veterans Administration's Psychiatric Evaluation Project.

[2] The author expresses appreciation to H. B. Thompson, M.D., Patricia Tomsich, R.N., and to the Nursing Assistants who rated patients on ward behavior.

TABLE 1
Comparison of Groups Before Treatment

Variable	Meprobamate	Prochlorperazine	Chlorpromazine	Perphenazine
Mean age on admission	40.1 (31–51)*	39.4 (30–63)	34.8 (21–52)	35.6 (22–52)
Median number previous hospitalizations	1 (0–7)	1 (0–5)	1 (0–2)	2 (0–8)
Mean months previous hospitalization	3.2 (0–19)	4.0 (0–23)	11.4 (0–60)	13.4 (0–85)
Mean age at first hospitalization	34.7	32.3	30.6	30.5
Number married	6	5	2	6
Mean school grade completed	9.6	10.2	9.8	9.9
Admission Diagnosis				
Psychoneurosis	6	6	4	3
Psychosis	1	4	12	12
Character Disorder	3	0	1	2

* The range is given in parentheses.

treatment on a prescribed basis other than assignment to activities.

Before treatment was begun, and again after four weeks on tranquilizers, each patient was administered the Minnesota Multiphasic Personality Inventory (MMPI) and was rated by a trained nursing assistant on the Behavior Rating Sheet (BRS).[3] The MMPI, which is part of the routine admission procedure, provided measures on 14 scales: three validity scales, L, F and K (which reflect the patient's test-taking attitude and the extent to which he is able to follow instructions), and 11 clinical scales, Hypochondriasis (Hs), Depression (D), Hysteria (Hy), Psychopathic Deviate (Pd), Masculinity-Femininity (Mf), Paranoia (Pa), Psychasthenia (Pt), Schizophrenia (Sc), Hypomania (Ma), Social Introversion (Si) and the Taylor Manifest Anxiety Scale (MAS). The BRS provided information on the ward adjustment of the patient. The scale was being used in connection with another study and did not constitute any differential treatment for patients in this study.

There were three criteria for selecting patients for the study. Subjects were all male veterans who had not been hospitalized and had not received tranquilizers on an out-patient basis during the preceding three months. They had to be able at the time of admission to complete the MMPI and had to continue on one of the experimental drugs for at least four weeks. Patients were admitted to the study over a nine-month period. At the end of that time patients in each drug group who met the criteria for inclusion were compared with patients in the alternate drug group. It was found that the groups were not comparable with respect to chronicity of illness. Therefore, patients were eliminated in pairs, the most chronic from one drug group and the least chronic from the alternate drug group, until there was no significant difference between the groups on this variable. Also, for the groups being compared there were no significant differences in median score on the BRS, mean scores on the MMPI scales, or on several other selected variables. There remained 54 patients, ten each on meprobamate and prochlorperazine, and 17 each on chlorpromazine and perphenazine. Comparison of the groups before treatment is presented in Table 1.

For each separate drug group, comparisons were made between pre-treatment and post-treatment measures. For comparable drug groups, comparisons were made of the patients' movement to open wards and out of the hospital. It was predicted that all groups

[3] This scale was designed for use with the Veterans Administration's Psychiatric Evaluation Project and is based on the Hospital Adjustment Scale of McReynolds and Ferguson.

would show decrease in pathology as indicated by decrease on all scales of the MMPI except K, which would increase), and that ll would show improved ward adjustment s indicated by a decrease in ratings on the BRS; but that for comparable drug groups) there would be no significant difference n the amount or kind of improvement, 2) here would be no significant difference in novement of patients to open wards or in he release of patients from the hospital, and 3) there would be no significant difference in the uniformity of action of the drugs.

RESULTS

Pre-treatment and post-treatment MMPI profiles for each group are presented in Figures 1, 2, 3 and 4. As predicted, the K scale was the only MMPI scale showing a significant increase in mean score following treatment for any of the drug groups. The increase was significant for the meprobamate

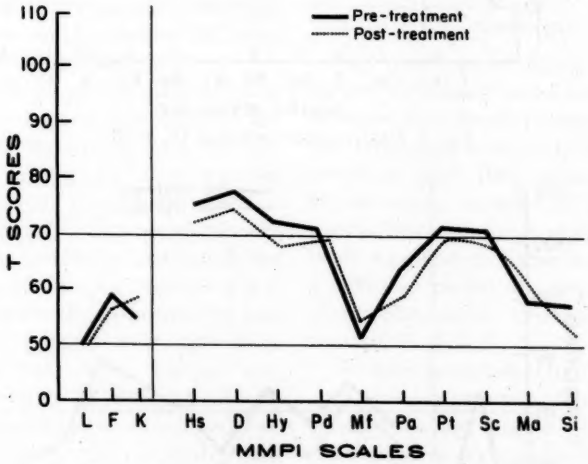

FIG. 1. Meprobamate Group.

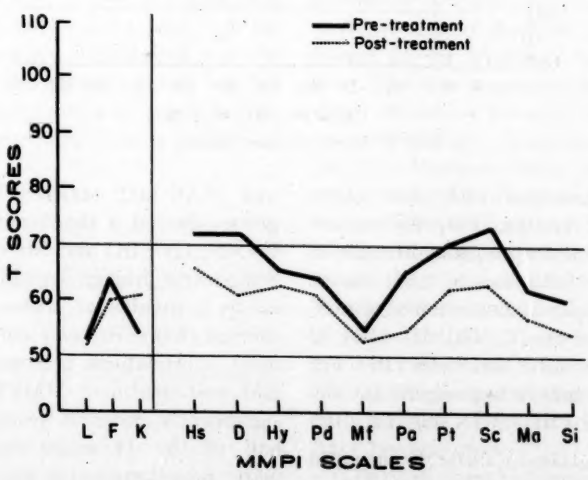

FIG. 2. Prochlorperazine Group.

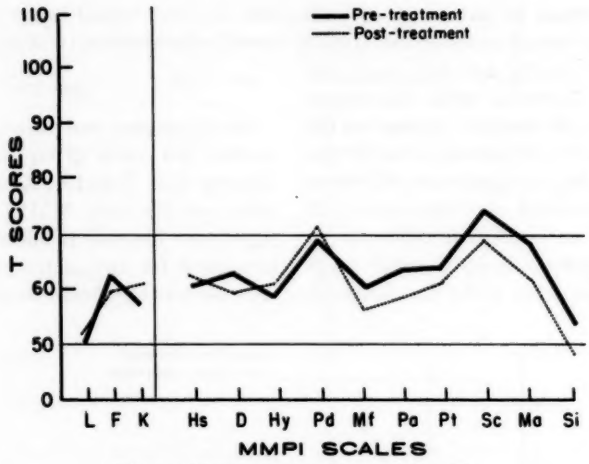

FIG. 3. Chlorpromazine Group (N = 16).

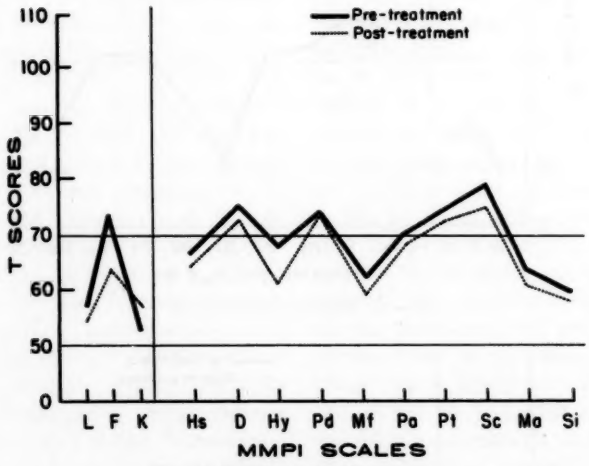

FIG. 4. Perphenazine Group.

(.05),[4] prochlorperazine (.01) and chlorpromazine (.01) groups. Patients on meprobamate showed a significant decrease on the D (.05), Pa (.02) and Si (.01) scales. Those on prochlorperazine showed significant decreases on the F (.05), Hs (.05), D (.05), Pa (.02), Sc (.01) and MAS (.01). For chlorpromazine there was a significant decrease on the D (.01), Mf (.05), Pa (.02) and MAS (.02) scales. The perphenazine group showed a significant decrease on the F (.02), Hy (.01) and Mf (.01) scales. The comparable drug groups did not differ significantly in number of scales showing a significant change in mean score following treatment. Correlations between pre-treatment and post-treatment MMPI scores were not significantly different from zero on about half of the 14 scales for the meprobamate, prochlorperazine and chlorpromazine

[4] The levels of significance are for the one-tailed test.

groups. For the perphenazine group, correlations were statistically significant for all scales except one.

After four weeks on medication only the prochlorperazine group failed to show the predicted decrease in median score on the BRS. Differences in ratings on the BRS before and after treatment were not statistically significant for any of the drug groups. Although the number is small, more patients on prochlorperazine than on meprobamate were rated as worse following treatment, and more patients on chlorpromazine than on perphenazine were rated as worse. BRS results are summarized in Table 2.

The median number of days that patients remained on closed wards was 45 days for the meprobamate group, 41 days for the prochlorperazine group, 91 days for the chlorpromazine group, and 77 days for the perphenazine group. The differences here are not significant. At the end of one month the number of patients on open wards did not differ significantly for the meprobamate and prochlorperazine groups, but did differ significantly for the chlorpromazine and perphenazine groups. At the end of one month, one chlorpromazine patient and four perphenazine patients were on open wards.

At the end of three months there was no significant difference between the groups in the number who had been released from the hospital. It was originally planned to study the patients for three months, but by that time only a small number of patients were still on the experimental drug. A breakdown of the groups three months after admission is given in Table 3.

TABLE 2

Scores on the Behavior Rating Sheet

Variable	Meprobamate	Prochlorperazine	Chlorpromazine	Perphenazine
Pre-treatment median	0 (0–4)	0 (0–8)	3 (0–9)	0 (0–10)
Post-treatment median	0 (0–1)	2 (0–4)	2 (0–11)	0 (0–5)
Number worse after treatment	1	3	6	3

TABLE 3

Drug Groups at Three Months

Variable	Meprobamate	Prochlorperazine	Chlorpromazine	Perphenazine
Out of hospital	8	6	4	5
On experimental drug	0	2	8	5
On different drug	0	0	3	5
Drug discontinued	2	2	2	2

DISCUSSION

The question of which tranquilizer to choose of the many now available can best be answered by comparative study of the drugs. The number of subjects in this study was of necessity small. Here, as at other hospitals, an increasing number of patients are on tranquilizers prior to hospitalization. On the basis of published studies, four weeks seemed a sufficient period of time within which to expect measurable changes as a result of therapy (1, 5, 7, 9).

With few exceptions (10, 11), meprobamate and prochlorperazine have been recommended for use mainly with patients with mild psychiatric disturbance. Of the five patients in these drug groups who had an admission diagnosis of psychosis, four became worse and the fifth one showed little change in his condition up to six months later. The view has been expressed that these drugs should be prescribed on the basis of symptomatology, regardless of diagnosis (10, 15). However, these findings suggest a need for further study of the use of these drugs with psychotics. With the group of newly admitted, severely disturbed patients, results of chlorpromazine and perphenazine were less than would be expected on the basis of earlier enthusiastic claims (7, 9, 14, 16). In about one-third of the cases, medication had to be changed or discontinued because the condition of the patient was judged to have become worse.

The prediction that drug groups would not

differ significantly in amount of improvement was confirmed. The findings lend support to the view that the tranquilizers affect symptoms differentially, producing greater improvement in some areas of functioning than in others (3, 14). Though all the tranquilizers are reported to be most effective in reducing overt anxiety and in calming agitated patients, only prochlorperazine and chlorpromazine patients showed a significant decrease in anxiety as measured by the Manifest Anxiety Scale. All groups but perphenazine showed a significant decrease on the Depression and Paranoia scales. This finding is of interest in view of the fact that depression is sometimes considered a problem with tranquilizers, and with chlorpromazine in particular (8, 11, 14). The significant decrease on the Paranoia scale for the chlorpromazine group is consistent with findings of another controlled study (13).

The significant decrease in withdrawal and paranoid ideation for the meprobamate group is consistent with the clinical finding that, with meprobamate unsociable patients become less withdrawn and again able to take part in the life of the community (1). Prochlorperazine is reported to furnish relief from such psychotic manifestations as delusions and hallucinations (11). Patients in this study showed a significant decrease on the F, Pa and Sc scales although, as mentioned above, they appeared worse behaviorally. Unlike the other drugs studied here, perphenazine appears to have a mild overall calming effect.

Meprobamate has been reported to be relatively uniform in its action, while the action of chlorpromazine has been described as unpredictable and variable from one patient to another (4, 12). Both the changes in ratings on the BRS and the lack of significant correlation for many of the MMPI scales give evidence that some patients derive more benefit than others from these drugs (6, 16). In this study there appeared to be no significant difference between meprobamate and prochlorperazine in their uniformity of action. But results do indicate that patients are much more variable in their response to chlorpromazine than to perphenazine. The high positive correlation between pre-treatment and post-treatment MMPI scales for the perphenazine group shows that patients as a group responded in a similar manner to this drug.

SUMMARY

This study was a controlled evaluation of the relative therapeutic effectiveness of meprobamate and prochlorperazine in the treatment of mildly disturbed mental patients, and chlorpromazine and perphenazine in the treatment of severely disturbed patients. Patients were male veterans, new admissions to a neuropsychiatric hospital who had not received tranquilizing medication for at least three months prior to admission. Patients were evaluated before and after treatment by means of a personality inventory and a behavior rating scale.

For comparable groups of mildly disturbed mental patients, meprobamate and prochlorperazine differ in therapeutic effectiveness. Meprobamate seems to have a greater tranquilizing effect on overt behavioral manifestations, which resulted in improved social relations on the part of the patients, while patients on prochlorperazine showed considerable improvement in mental symptoms. Compared with perphenazine, chlorpromazine appears to produce more variable effects on patients, which suggests that patients should be carefully chosen for treatment with chlorpromazine. Chlorpromazine seems to be much more effective than perphenazine in reducing depression and paranoid mentation. As compared with chlorpromazine, patients on perphenazine showed more improvement in ward behavior, but less improvement in mental condition.

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A PSYCHOLOGICAL APPROACH TO TRAUMATIC PARAPLEGIA: USE OF HYPNOSIS

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If hagfish can overcome the effects of spinal section, if a rabbit can use a paralyzed leg under intense stimulation, and a paraplegic monkey run before he can walk; is the case for so-called spinal man as hopeless as it has appeared?

This paper presents the view that, in man, paralysis may be understood as a function of personality, and that as such its psychological treatment may offer a new and hopeful method of attack. Two cases of clinically diagnosed complete paraplegia treated with hypnosis will be discussed as illustration.

Although the term "complete paraplegia" seems to imply a complete severance of the spinal cord, in practice the diagnosis is based on such clinical evidence as complete loss of motion and sensation below the level of the lesion, loss of sphincter control, pathological reflexes and other criteria. If recovery does not take place within a year the condition has been assumed to be permanent and irreparable. That voluntary control may still be possible even in long-standing cases, attributed to residual motor connections not provoked by the usual clinical tests, Kabat (15) has demonstrated, using physiological stimulation. He calls this central facilitation and tacitly recognizes the psychological element by observing that the most important ingredient in the program is the psychotherapeutic effect of intensive exercise to achieve what had not been thought possible.

When one notes that identical lesions may result in either a flaccid or spastic type of paralysis, and that spasticity may be either in flexion or extension (7), and when one finds that maximal histologic damage has been reported with no paralysis, as well as maximal paralysis with slight anatomical damage in animals (6), it is appropriate to speculate as to what other factors besides the tissue damage account for such diversity. An evaluation of emotional factors may change the concepts of this syndrome, just as each advance in technique has brought a change.

In 1890 Bastian (2) thought that the lower half of the cord must be dead because no reflexes appeared (and indeed most patients died *in toto* before this theory could be disproved.) Head and Riddoch (11) then discovered that toxemia was responsible for the depressed reflexes and their studies on the automatic bladder proved that the lower cord could function even though removed from consciousness. They reasoned that the mass reflex was a primitive type of response signifying that the cord was wholly severed, but that extension reflexes, representing a standing or higher type of reflex, meant that the cord was only partially severed. These concepts had to be discarded when evidence from World War II (7) showed that the type of reflex bore no direct relation to either the level or severity of the lesion. The notion of the necessary permanency of the condition has been dispelled by Kabat's work. Emotional factors as yet have not been adequately considered: what part do they play in the paralysis; what effect may they have in overcoming paralysis.

BACKGROUND

Neurological: Campbell (5) found that conduction across actual gaps in structure appears to be possible under certain conditions

[1] 161 Kuuhoa Place, Kailua, Oahu, Hawaii. This report is based on a thesis submitted to the University of Buffalo in partial fulfillment of the requirements for the Master of Arts degree. Thanks are due to Professor Carleton F. Scofield, sponsor of the study; to Dr. Samuel Yochelson for conducting the hypnosis and psychotherapy; to Rolf Flygare for physical therapy; and to the New York State Bureau of Rehabilitation for underwriting the original investigation.

in the hagfish. Even when three cuts were made in the spinal cord, if the animal were persistently handled it could take off on swims which were essentially normal. Campbell (6) found that rabbit, when experimentally paralyzed to simulate poliomyelitis, under sufficient stimulation would at times take a few normal steps with the affected limb before relapsing into the paralysis. The intensity of the stimulus or the level of excitement of the animal in question appeared to be the important factor. Campbell believes that this calls for the inclusion of the concept of organism-as-a-whole into paralysis and spinal dysfunction.

Certainly knowledge of the functioning of the cord proper is far from complete. Lloyd (20), who studied action in the cord by electronic methods, stated in 1944 that virtually nothing was known of the unmyelinated fiber systems of the cord. He discussed the role of extrasynaptic transmission, mentioning action due to fields of current and chemical diffusion gradients within the neurophil about which little was known.

In his book *The Organism*, Goldstein (10) proposes a theory of neurological function which views any muscular movement as a behavioral adjustment, a relation between the capabilities of the organism and the demands of the environment.

From a practical standpoint the recovery of damaged parts where function would seem all but impossible is within the experience of many practicing physicians.

Emotional facilitation: Tower (28), in her monumental studies of pyramidal tract function, notes that after unilateral section in monkey, if the animal were angered or otherwise emotionally aroused, it could use its weak arm nearly as well as the normal one. "It appears, therefore," she says, "that the effort and the fatigue are largely one of the will," a strange word for her to use. In bilateral section where the effects are more grave, *i.e.* complete paralysis, the monkeys eventually learn how to walk, whereas in unilateral section where the good arm can compensate for the weak one, recovery is slight. To the observer a relation between need and amount of recovery is suggested. And one wonders what mechanism could be called into play in the monkey learning to walk who loses his balance and takes a few perfectly coordinated running steps before he falls.

Again, while Tower (27) feels that the finely executed phasic movements are permanently lost after pyramidal section, she excepts one cat with an ulcer behind its ear who after many trials finally succeeded in producing the desired phasic motion of scratching, an instance where under the stimulus of excessive need the ordinarily impossible act is achieved.

Mental components in human paralysis: Sister Kenny (21) has used the terms "mental alienation" and "loss of mental awareness" to describe a condition in poliomyelitis where a muscle appears to be paralyzed but actually is not. The ability to move it seems to be blocked out of consciousness. Bohnengel (3) theorizes that this is a dissociative mechanism under the highest level of integration. He feels that this is a psychological reaction to muscular immobilization and that its basis is not in the cellular pathology but rather in the psychologic organization of the individual.

In a traumatic spinal paralysis the patient goes into a state of shock where the total musculature below the point of trauma becomes dramatically and instantaneously cut off from awareness and motion. If these phenomena are reactions to immobilization a more fruitful field for their growth could scarcely be imagined. Since the paraplegic case involves an added sensory loss, a more intensive therapy is indicated than that of the Kenny reeducative method.

The body-image is a subject rich in implications here. Schilder (23) defines it as the schemata a person carries in his mind against which postural alteration is measured, motility flowing through it, being guided by it and in turn modifying it. Of all the senses contributing to it he gives special importance to

the vestibular apparatus concerned with gravity. Perhaps the passive swinging which Cameron (4) reports to be of inestimable value in recovering cords is concerned with the orientation to gravity. The component of sexuality involving relations to external love objects also makes an important contribution to the formation of the image so that it seems possible that emotional attitudes could influence motility by way of their impingement on the body image.

In treating a case of hemiplegia with hypnosis, regressing the patient to an age before the paralysis, Shiries, Peters and Krout (25) found that at first the patient could not form a mental image of herself kicking a ball. They remark that she seemed to be paralyzed mentally as well as physically. Recovery started after she was able to visualize the motion. Another case of hemiplegia in which hypnotic age regression was used to reduce spasticity and increase range of motion has been explained by Higley (12) in terms of a form of mental alienation.

Unconscious determinants: Kubie (17) has discussed the way in which an injury may activate unconscious levels of activity and assume symbolic meaning to the individual in terms of his psychologic makeup. He quotes Cushing, who made this observation in 1918 on patients with low transections of the cord: "It was as though the elimination of the influence of all the subtly disturbing impulses from the sacral region had removed from the patient a major source of discontent, of tension and of energy. Thus, the anatomical dissection made by the bullet achieved permanently and with precision the 'belle indifference,' the happy acceptance of invalidism, which a hysterical dissociation can accomplish only approximately and temporarily."

In our view elements of purposiveness actually may have been present.

A case of Kupper's (18) illustrates how unconscious conflicts can be mobilized by injury and how the release of emotional pressure can serve to relieve symptoms. Under hypnosis a young seaman, whose epileptic seizures apparently resulted from a head injury, related his seizures to the word *hate* and described a specific scene at the breakfast table where the father had hurled a cup of coffee at the mother generating intense feelings of hostility in the patient. Seizures could be induced or stopped by the operator only by manipulating the material brought out in this scene. Seizures were recorded by EEG and spikings were found to occur in conjunction with the psychological material. Through psychotherapy the patient became aware of the connections between his feelings and his attacks, with consequent abatement of symptoms.

Kupper believes that his cases represent total personality changes due to combined organic and psychic injuries. He believes that during a period of unconsciousness psychic mechanisms may attach themselves to present or pre-existing pathology exaggerating and distorting it. The patient regresses to a state of equilibrium which can become permanent unless the repressed emotions are released and understood by the patient. Kupper remarks on the strong dependency-passivity needs in these individuals.

It is of more than passing interest that Thom, Van Salzen and Fromm (26) in psychological studies of 109 paraplegics note that the disability established feelings of dependence on others which tended to persist beyond the needs of the disability, a state which the authors say is frequently recognized as regression.

It seems possible to this writer that many cases of complete paraplegia, clinically diagnosed, may in fact represent combined organic and psychic injuries in which a state of equilibrium has been reached at the expense of the body in the exaggeration of the functional paralysis. By approaching the problem psychologically, the mental component may be altered in such a way as to allow reintegration with a resultant extension of motor control.

CASE STUDIES

CASE ONE

This is a thirty-year-old woman paralyzed from the waist down for nine years, a paralysis which became spastic after seven years. Driving home from a midwestern university, she was in an automobile accident in which compression fractures of the 7th and 10th dorsal vertebrae were sustained. There is an amnesia for the accident. Paralysis set in immediately, with complete loss of motion, sensation and sphincter control below a level at 10D. Delirium, high fever, profuse sweating of the upper body, pain, projectile vomiting, and the formation of seven draining decubitus ulcers marked the first three weeks. During the recovery period the patient did pull-ups on an overhead trapeze and other exercises on her own initiative. In the sixth month, the patient was taken to the Neurological Institute, New York City. Report of laboratory examination: Spinal fluid—2 cells, one plus globulin, 50 mgm. protein; Gold Curve 1111100000; negative Wasserman; manumetric-partial block.

In the seventh month daily massage was started and reflexes began to appear. The following summer the patient swam and performed exercises in a nearby lake, acquiring the kinesthetic sense of moving her legs. A definite thrust was felt by the assistant. Then both motion and sensation were lost during the months that an indoor tank was being built, but were regained somewhat with the aid of a physical therapist. At the end of two years the patient was standing up daily in a three-sided walker without braces. Knees locked in the upright position and the patient attempted to "walk" by swinging the flaccid legs from the hips so that the feet alternately slid along the floor a few inches at a time.

At first the patient, showered with attention, drifted along not at all unhappily. By the end of the first year the significance of the injury appeared to have been realized and accepted; she became interested in art, music, philosophy, and went to parties and concerts. This period was culminated by becoming engaged to a young man known formerly in college, who felt that the disability was no bar to marriage.

The patient increased attempts to "walk," but one day collapsed exhausted, four years after the injury. The patient was then taken to the Mayo Clinic for evaluation. The prognosis was that there would be no return of motion or sensation. She was fitted with braces and crutches and taught to ambulate in them. After talking with doctors at the clinic, the financé's attitude changed. He entered military service, was sent overseas and not heard from again. The patient went into a state of depression and gradually became weaker. Exercises and massage were given up and she remained in bed nearly a year and a half. She finally decided on a new course, resumed brace walking until she could cover nearly 100 yards in a fast swing-through gait, and approached the New York State Rehabilitation Bureau where she received financial aid in carrying out a long term program to finish her education. In preparation, she went to the Institute for the Crippled and Disabled in New York City for further physical rehabilitation.

Development of spasticity: While practicing creeping the knee seemed to spring forward of its own accord. Soon the left side tightened up so that the patient could not sit on the floor without tipping over. Within a period of two months her legs began to jerk so much that she could not stand in braces. Back at the Institute, doctors could offer no explanation for the spasticity. One neurosurgeon,[2] head of a paraplegic unit, observed that in his experience spasticity had been known to develop in the first year, changing over to voluntary control, but never so long after an injury.

The patient returned to college in a wheelchair and was graduated with honors in psy-

[2] Dr. A. T. Jousse, Lyndhurst Lodge, Toronto, Canada.

chology. She enjoyed the college work, and adjustment now seemed satisfactory.

Situation prior to hypnosis: Spasticity, developing seven years after injury, had been in force for two years. Strong adductor tension held the knees tightly together at all times. They could be separated a few inches by force but would clamp together when released. If the legs were manipulated they would go into strong extensor spasms. In siting, the left leg spontaneously tended most often to go into flexor spasm, the right into extensor spasm. A neuropsychiatrist, consulted to attempt hypnosis, expressed skepticism in view of the unequivocable pathology but felt that the unusual development of the spasticity and the determination of the patient were hopeful signs.

RESULTS WITH HYPNOSIS

Reduction of spasticity: Using spot fixation a light trance was achieved in the first session. Before hypnosis in the second session, the right leg kept going into extension spasms while the left leg was drawn back against the chair so tightly that the physician was not able to pull it forward. The patient went into a trance quickly. At the command, "Relax your left leg; you can relax it; it will relax," the leg loosened and could be swung forward easily. On awakening, the left leg remained relaxed and could be flexed and extended without resistance. It remained so for two days while the right leg stayed stiff and resistant. Extensor spasm in the right leg could not be broken by force either by the psychiatrist or by a witness before hypnosis in the third session. On the command to relax, the right leg released and both legs could be flexed and extended together or separately like pendulums. They could be spread apart widely without resistance. The patient came out of hypnosis feeling exhausted, evident only at this and the previous time. The legs remained completely relaxed for about a week. Then tension returned somewhat and was especially noticeable at certain times under certain conditions but at no time did spasticity ever approach the pre-hypnotic state. The patient estimated a reduction of perhaps 70 to 80 per cent.

Motion under hypnosis: In the fourth hypnotic session the patient was told, "You can raise your right leg, lift it from the floor yourself." The foot responded by lifting about an inch. In a sitting position it was eventually possible to raise either foot from the floor so that the knee was in full extension, and lower on command in a controlled motion. Flexion was also achieved, pulling the foot backward along the floor. The right big toe responded to commands to bend up and down alternately although the left did not. *Under hypnosis the legs would go into spasm on command and assume the original position of right leg in extension, left in flexion. They would then relax on command* allowing themselves to be rotated in a fast running movement with no resistance. This was very disturbing to the patient and was only done a few times.

Sensation under hypnosis: Occasionally the kinesthetic sense of motion was very clear. A pinprick was localized correctly on the outer aspect of the left leg above the knee. At one time a tuning fork was vividly felt as vibration at the knee and ankle.

Voluntary motion in the waking state: Physical therapy was started in the fourth week, but not until the fifth month did a flicker of motion appear. Lying on the side with the leg flexed slightly, the lower leg could be extended so that the foot swung through an arc eventually of 6 to 8 inches. The hip could be flexed to move the knee upward about an inch or two. Slight extension of the hip started and occasionally the foot itself could be dorsiflexed. Simultaneous abduction of both legs could be performed. An acute burning sensation would be localized in all muscles which were attempting to contract.

Brace walking: The legs did not go into spasm when braces were worn. A residual tightness remained in the hip flexors which could be overcome by standing quietly in

parallel bars. The patient learned to progress in crutches and braces about 50 feet, swinging one foot at a time.

Psychological results: In the sixth hypnotic session the patient was asked, "Why did your legs become spastic? Why did you want them to tighten up?" She twisted from side to side as if the questioning were painful. When asked what she was thinking about, she replied, "I want him." "Who"? She then replied with the name of her former fiancé.

Further talks under hypnosis revealed how the wish to have the legs change was related to the patient's feelings after the fiancé had left. His leaving was construed not only as a rejection of herself, but as a rejection of herself in the flaccid state, of a capability of loving or being loved. It was difficult for the patient to accept consciously the idea that a need for affection existed; yet under hypnosis, "The legs had to change, they had to have motion," *i.e.*, a different state had to be reached.

Questioning under amytal revealed that adduction was a position assumed as protection against the fear of exposing inner needs. When asked why the legs had assumed the positions into which they had gone, she replied, "I want him but I want to be rid of him (psychologically). I'm pushing and pulling at the same time."

The relation of tension in the legs to emotional tension was often noted. In relating disturbing material the legs would visibly tighten at key points; *e.g.*, in relating a death dream in which the significant problem was again revealed. In a classroom or in a strange group the legs would be much more likely to start involuntary kicking than at any other time.

During the sixth month of treatment, when brace walking was started, the patient was helped to her feet under hypnosis. On command to relax an upright position was attained. When told, "Your eyes will open and you will be awake," the hip flexors went into a spasm pulling her over into a cramped position. Under hypnosis she spoke of a reluctance to stand straight, to face a changed situation. "I want to walk but I'm afraid, too."

Toward the end of this study the command to lift the foot met with only a slight response. With the outpouring of current troubles, the same command elicited a much greater range and strength of movement.

DISCUSSION

This case is remarkable in that a measure of voluntary control and rudimentary sensation were established primarily by psychological methods in a paralysis of nine years duration. The time element is especially striking since such an occurrence would generally be held impossible on the basis of disuse atrophy alone.

Some points are obscure. It cannot be established how great an organic injury was actually sustained because the cord was never examined directly. However, the immediacy of the paralysis, the gravity of the vertebral fractures, analysis of spinal fluid, slow appearance of reflexes, and the complete absence of sensation, motion and sphincter control would indicate a severe cord injury by accepted clinical standards. In order to explain the late spasticity and change to a measure of voluntary control with hypnosis two explanations seem possible: 1) either the cord was not as badly damaged as it was thought to be, in which case the original symptoms must be considered at least partially psychological in origin, or 2) structural changes, correlated with emotional changes, took place in the cord many years after injury. The two hypotheses are not contradictory nor, of course, mutually exclusive.

The fact that the potential for voluntary motion may have been present from the beginning is born out by the appearance of motion when exercises were carried out in the lake and later in the indoor tank. But with the weight of the limb eliminated, why

were results so slight at this time when the muscles were relatively firm and healthy? Under hypnosis the foot lifted against gravity nine years after injury. One wonders what might have been the outcome if effort had been enhanced by hypnotic suggestion at the earlier time. One is also compelled to wonder what emotional deterrents might have been revealed if an assay of unconscious motives had been made at that time.

It seems reasonable to assume that the trauma of a spinal cord injury disrupts not only the physiological integration of the individual but the total personality integration as well. Psychologically the person's conception of the world and his place in it must sustain a shock, as well as his body. A new integration must be made if the person is to survive at all. The systems which carry into effect the aims and purposes of the individual will reach an integration in line with the new changed aims and purposes of the injured individual. From a psychological standpoint a singularly happy adjustment was reached by this patient in the years immediately following the accident. At the same time a drive to overcome the disability existed on a conscious level. In spite of conscious efforts, other factors clearly must have been present which prevented the available neural structure from being used. As a conjecture, certain conflicts may have been resolved by the injury, and the connection between conflicts and symptoms existed beneath the level of awareness.

Although an objective estimate of the pretraumatic personality cannot be made, it is of passing interest that the acute symptoms of the traumatic syndrome as outlined by Kardiner (16) were present: delirium, terror reactions, sensitivity to stimuli, irritability to auditory stimuli. But it is questionable whether a traumatic neurosis developed, in view of the seemingly strengthened rather than weakened ego. Anxiety, if present, must have been completely bound up in the organic symptoms. In Kardiner's descriptions of types of neurosis, those with a sensory-motor paralysis did not suffer from feelings of tension. That psychic tensions may convert to bodily symptoms is, of course, well known, but adherence to the neurological pattern obscures the simulation when an actual injury is present. Freud (9) brings injury into the libido theory by observing that a body deformity may often dissolve a psychoneurosis by gratifying the need for punishment arising from a sense of guilt. Menninger (2) follows along the same line in discussing self-inflicted injuries. The psychosomatic relationship is extremely intricate and perhaps it would be simplest to say, as Kupper has, that an organic injury apparently can serve as the nucleus for the resolution of conflicts.

In the case of this young woman dependency needs were certainly catered to. Being the center of attention may well have satisfied a deep-lying need for security in such a way that invalidism became a solution to a problem.

Whatever the particular mode, integration was sufficient to meet the needs that existed; motivation was not strong enough to break the imposed pattern. Then a change did take place. In the light of the psychological material it is apparent that another trauma was sustained, one in which psychological elements predominated. The second trauma was experienced as a breaking up of the self. Out of this disintegration a new attempt at balance was striven for and in the new attempt the unconscious will to achieve motion seems to have played an outstanding role. Motion became a choice between life and death. The strenuous exercise at the time spasticity developed no doubt played a precipitating role, yet it alone cannot account for the change. Strenuous exercises had been performed previously when braces were first used at the Mayo Clinic, but spasticity did not result.

When questioned under hypnosis, the patient said, "As long as I thought about it I was all right, but when I pushed it out, it went into my legs." At the time she started

exercises, the depression was lifting but the old episode was not truly forgotten and its effects continued to exist dynamically in the spasticity. Under hypnosis, tension, which had been present constantly for two years, released instantaneously. There does not seem any alternative other than to assign an involuntary, unconscious origin to the spasticity: the psychological material is too much to the point to be dismissed as incidental.

The discussion thus far has been from the point of view that connections were present in the spinal cord from the first but that their usability was prevented by an unconscious block; the potential was then revealed in the second trauma which disturbed the psychological organization to the extent that indications of a recovering cord began to appear.

A second possibility is that changes took place in the spinal cord many years after injury and that these changes were correlated with emotional alterations.

If normal innervation comes about in a pattern that is geared to the demands of the situation, then it may be possible that the deep need to change, which became a part of the patient's life as a result of the rejection by the loved person, so influenced the innervation pattern that changes in the nervous structure took place. Jacobsen (13) has reported that action currents can be recorded in specific muscles when a movement of those muscles is merely imagined. Young (30), in relation to the atrophy or hypertrophy of a nerve, says, "The fact that the structure of the fabric of the body is modified by use is so familiar that we perhaps do not sufficiently often pause to consider how amazing it is. The self-preserving actions of the body are surprising enough in themselves, how much more so the fact that the living system is one in which each part increases or decreases in size according to the demand placed on it."

In studying the return of motor power after cerebral extirpation in monkey with the use of drugs which stimulated or retarded impulses, Watson and Kennard (29) came to the conclusion that the functional reorganization of pathways depended on the volume of impulses passing over the available structure and further that this might be due to the myelinization of previously nonfunctional axons.

Now if nerve structure can be changed by stimulation, if nerve tissue develops according to the demands placed on it, and if reorganization depends on the volume of impulses; can we infer that a need so intense and "demanding" that it could not be tolerated at a conscious level, was capable of developing cord tissue or reorganizing tissue that had been disrupted? Certainly physiological changes accompany strong emotion and it is known that many systems are susceptible to chronic stimulation.

The argument that changes took place without awareness holds here too. That any connection exists between the situation which the patient was trying to dismiss by pursuing other activities, and the uncomfortable spasticity, was most surprising to her when it was brought out under hypnosis. One might speculate that this was an example of the mechanism of suppression, and that the spasticity had appeared as an unconscious conflict resolution.

Some points are difficult to explain, as the discrepancy between the amount of motion under hypnosis as compared to the waking state. If hypnosis is the royal road to the unconscious, as Erickson (8) has remarked, some barrier must still remain in the conscious state.

The basic problem of what happens in the process of hypnosis is central here. The theory that the person is striving to act like a hypnotized subject seems not well supported in this instance. Arnold (1) offers an explanation in terms of focused imagination based on a relation between thinking and motor phenomenon, from her work on body sway and mental imagery. Arnold's theory is appealing because of the importance of

establishing the link between thought and motor processes, not only as a means of explaining changes as a series of minute stimulations, but also as it might lend itself as a method in a retraining program. Arnold postulates the central initiation of sensory perception as a necessary assumption in her theory, a point which has a bearing on our second hypothesis; that structural changes may be correlated with will or thought processes. For if an individual can direct the way he perceives stimuli, as Arnold believes he does in hypnosis, then the ascendency accrues to the thought processes. A measure of self-initiation becomes a possibility in the creative production of changes in the body.

Symbolism: In regard to the symbolism shown by the form of the spasticity, one is reminded of the formulations of Smith Ely Jelliffe (14). In discussing the Parkinsonian posture he noted that while organic lesions have great significance, some people with localized lesions recover and some do not, a point pertinent here. Jelliffe thought that the participation of unconscious factors brings about the ambivalent posture of recoil and challenge, the typical bent posture being but a partial defense against the real outward expression of rage.

For the patient here discussed, the strong adduction of the spasticity appears too as a defense against reactions which cannot be expressed. The bizarre position of the legs, one in flexion, one in extension, seems to represent an underlying ambivalent attitude. The remarkable thing is the way the injured motor system has been used to express the affective attitudes.

CONCLUSION

In this case of longstanding clinically diagnosed complete paraplegia, the change in the organic condition many years after injury becomes to some degree understandable in the light of the psychological events of the intervening years. The time lapse reveals a number of pertinent issues with striking clarity and makes necessary the admission that unconscious emotional factors participated in the motor symptoms. The change in innervation cannot be understood without acknowledging the complex relationships of the organism-as-a-whole, relationships which can best be described in terms of "meanings" and "purposes" for the individual.

CASE TWO

This is a 19-year-old girl with a complete paralysis below a line breast-high as a result of a shooting accident three years previously. The girl was with several companions when a rifle accidentally discharged, striking her in the right shoulder. She fell to the ground unable to move her trunk and lower limbs. Neurological examination revealed complete paralysis of all muscles below the mid-thoracic and an absence of all sensation below the same level. X-ray showed a bullet lodged in the spinal column at the level of D4. An operation was performed and the bullet was found posterior to the dura. A small slit was noticed where the bullet had evidently pinched the dura which was then opened to observe the spinal cord. The hospital record states, "A small laceration was observed on the posterior surface of the cord but the relative *injury appeared to be minimal* (italics added) in spite of extensive physiological alterations, with complete loss of all function below the level of the lesion. A small opening was made in the arachnoid for further visualization of the cord injury which still appeared to be minimal from a gross standpoint". Hospital course was uneventful except for a suprapubic cystosomy performed to prevent urinary complications.

The patient recalls that the first spasm occurred in the left leg a week after the accident. Spasms increased in strength and frequency until in the seventh week the nurse had great difficulty in putting a pillow between her knees to prevent pressure sores. Profuse sweating was a constant feature for the first two years.

When the patient was first brought to th

hospital she refused to eat the hospital food, and only ate what her mother brought in from the outside.

At the end of eight weeks, she went home with a nurse in attendance. A pillow was placed under the knees to relieve extension spasms and gradually the legs contracted in flexion until they were drawn up tight to the buttocks. Traction was then employed and although the legs gradually straightened, a contracture remained in the last 20 degrees short of full extension which prevented the use of braces. Curare failed to relieve spasticity.

The patient was able to sit up for long periods, all day and late into the night. She preferred to spend her time in a dark room of a small photographic laboratory installed for her use rather than in meeting people who came to the house. She was described as always having been quiet and shy.

A few months before treatment with hypnosis started, the patient had been taken to a neurological center where a nerve crush had been recommended. She was in favor of the operation but her parents insisted that she wait another year.

Pre-hypnotic examination: When seen by the same psychiatrist who had treated the previous case, the patient appeared as an attractive young girl, blonde and blue-eyed, apparently well adjusted to her disability. At examination the total absence of sensation and voluntary motion were noted. Pathological reflexes were present. The clinical evidence gave the impression of a severe cord injury. When sitting in a wheelchair the legs would go into frequent spasms causing the knees to lift upward toward the chin and the lower legs to cross in a scissors-like adduction. The legs resisted passive movement by usually going into extension. If forcibly bent at a certain point the legs would relax and could be bent the rest of the range, a sort of lock and key spasticity. Strong adduction was present at all times. Back aches were frequent. The progression of the spasticity had followed the normal course and did not seem extraordinarily severe. The legs appeared to be in good condition with little or no atrophy.

RESULTS WITH HYPNOSIS

Sodium amytal was administered as a test trial to see if results warranted having the patient move to another city for further treatment. Under light dosage and on the suggestion that the legs could relax, spasticity disappeared. The legs could be swung back and forth in extension and in flexion and spread apart without resistance. The patient's parents said that no such relaxation had been achieved in the previous three years.

She stayed at a hospital in the city to which she was taken, but a private consultation room was not available so that conditions were far from ideal. Even so, in a light hypnotic state a pronounced relaxation could be obtained instantly. After treatment ended, however, the legs would revert to the spastic condition. The interval of relaxation varied, lasting several hours with hypnosis, overnight with sodium amytal. Over a period of three months spasms on the whole seemed less frequent and less intense.

Contractures at the knee were released under hypnosis so that braces could be locked in full extension, an impossibility previously. Later while standing in braces supported on either side, the patient reported that she had the feeling that she could walk off down the hallway.

In the third month, exercises were started at a rehabilitation center in another part of the city. The patient took a taxi there and also to the psychiatrist's office by herself. She learned to get in and out of a car without assistance.

Near the end of the treatment period, while lying down under hypnosis, she was told that she would move her left leg on the count of five. She responded successfully with a rather jerky movement which came five or six times on command. On this day, previous to coming, the patient had felt the

desire to urinate naturally and was very disturbed for fear she would wet herself enroute. The legs suddenly had become limp and remained so for some time before and after treatment.

The following week she was discharged home where plans for continued physical rehabilitation were to be carried out. According to the psychiatrist, the patient reacted to hypnosis and amytal with "schizoid characteristics." His questioning revealed a personality which he felt was emotionally undeveloped. Because he felt that there was no inner drive or positive response to psychotherapy, no attempt was made at personality reorganization. Hypnosis was continued for whatever value it might have as a suggestive technique.

DISCUSSION

In this case spasticity had developed according to the regular pattern, yet it yielded to hypnosis so that the legs became pliable and putty-like. This fact suggests that the over-abundant impulses pouring into the musculature may have been cortical in origin rather than due to another cause, such as a hyper-irritable reflex condition peripherally stimulated. If cortical, then an explanation based on an emotional component cannot be ruled out even though material relating to a specific problem was not obtained.

Since hypnosis brought relaxation only for a short period (in contrast to the earlier case), it might be postulated that organic factors played a more decisive role here. But this does not seem likely in view of the surgical report of a minimal injury. It is possible, too, that hypnosis broke through the surface symptoms of the personality structure; here the spasticity is regarded as a dependent part of the total integration. The underlying structure was so deeply entrenched that when treatment ended the old pattern reasserted itself. The difference may be one of resistance to change; the first patient was actively seeking a solution whereas this subject was brought to the situation by her parents. Differences in transference may have been important.

Over the whole period of treatment the severity and frequency of spasms seemed to diminish. While in the hospital this latter patient made friends with the staff and even accepted an invitation of a former male patient to have dinner in a restaurant. She seemed happy to visit the writer's home. Travelling alone by taxi also represented a great gain in personal independence for a person who habitually withdrew from others.

Whether the change in spasticity was linked with a change in attitude is conjectural. However, the release of the contractures is another matter. The literature mentions hysterical contractures among British soldiers in World War I (24) which were relieved by suggestion. In this case the contractures appeared as an aftermath of a series of organic changes. But the fact that they could be dissipated by a hypnotic technique strongly suggests that other factors than physiological may have been important.

On the day that the patient's legs moved on command, she was in a state of terror and was amazed by the fact that the legs had gone limp "of their own accord." She felt subjectively that she had gone deeper under hypnosis than before.

Kupper holds that psychological changes take place during a period of unconsciousness. Although there was reported no actual period of unconsciousness, this patient experienced the bullet wound as an "electric shock" passing through her body. From conversations with the patient and her mother, it seems likely that there may have been complicating emotional factors connected with the injury itself. The patient disliked the boy who shot her and did not believe the shooting to be accidental. "He knew there was a bullet in the gun" when he appeared to be trying out the gunsight. She insisted that after she had been hurt the boy had come into her room and tried to choke her. While her exact relationship to the boy was not clear (except that she had had a "crush" on him at one time), her feelings about the acci-

dent are certainly linked with an interpersonal relationship of special significance for her.

It is possible that a complete disruption of the innervation pattern took place instantly. With incentive lacking, with the split mirrored in the schizoid reaction, the condition assumed permanency. A minor point is of some interest—the refusal of the patient to eat hospital food. This suggests that the injury served a purpose for the patient. The demand for feeding, appearing so strongly, may have reflected affectional needs which the injury as a whole could have taken over. The exaggeration of pathology and the helplessness might even be seen as a form of punishment towards the parents, making the older form of expression unnecessary. Certainly withdrawing tendencies were intensified. To all intents the patient is now anxious to walk again; she sees to it that her braces are put on and she actively plans for exercise periods. But according to the attending psychiatrist the "will is not there:" the internal motivation is missing.

CONCLUSION

If the laceration of the cord was minimal, as the hospital report stated, it is possible that with absorption and healing much of the cord might be serviceable. The fact that spasticity yielded to hypnosis which is confined to cortical stimulation substantiates this view. The injury was to the conduction fibers, although it is not impossible that co-ordination centers existing in the cord could have been disturbed in such a way as to produce the spasticity. If so, they too were amenable to cortical influence. As above, if the spasticity was cortical in origin, an explanation in terms of an emotional component cannot be dismissed.

IMPLICATIONS FOR REHABILITATION: SUMMARY

Trauma, whether physical or psychic, may effect the person at all levels of integration. When a paralysis strikes from the outside as in a traumatic paraplegia, it is assumed that the personality structure is profoundly modified by the disruption of the physiological integration and that a balance is attempted at a new level. Because of the indivisibility of the human organism it becomes theoretically possible again to modify the level of integration by psychological manipulation. We are concerned here with a psychological approach as a means of initiating changes throughout the organism.

One of the necessary functions of a rehabilitation program is to ascertain the meaning of the injury to the individual. It is entirely possible that the paralysis itself may serve a purpose in the life of the individual, that conflicts arising from his way of life have been resolved in the injury or in the exaggeration of pathology. Perhaps this may be especially applicable in those cases of complete paraplegia where the diagnosis has been based solely on clinical evidence, and the true extent of the injury not known.

A program of exercise to call out all available resources would be oriented to a view which sees personality developing in relation to the demands placed upon it, modified by the willingness or unwillingness to meet such demands. Kabat's system is an example of intense stimulation, "waking up the dormant neurones." Whole-body exercises such as swimming, creeping or rolling, which throw demands on the lower body at the same time that the body-as-a-whole is in action would seem to offer particular benefit.

One of the complications in paraplegia may be the presence of a spastic condition which in severe form can be incapacitating. Because of the irreversibility of surgical procedure employed to relieve it, other methods, as hypnosis, should be given a full trial.

The fact that spasticity has been shown to be reduced and contractures released in the hypnotic state, suggests that an emotional component may play a part in the genesis of spasticity. It is suggested that the position of the limbs of the paraplegic may be understood in terms of unconscious attitudes.

The psychosomatic basis upon which hyp-

nosis rests needs to be explored in detail and such serious organic cases as these should offer special opportunity for investigation.

In reeducation, the formation of effective mental images with hypnotic age regression, heightened stimulation with hypnotic suggestion, muscle training through focused imagination, all are possibilities. Since it has been found that unconscious attitudes may be symbolized through muscle tension, it is necessary to explore deeper attitudes in order to come to an understanding of control.

As to how voluntary control in recovering cords may be explained, Goldstein (10) has remarked, "How the excitation pattern comes about when a specific movement is intended, we cannot as yet determine with any degree of certainty. Here we are facing the problem of how to explain volitional performance in general. In this regard, we can say at this time only the following; the intention of a volitional performance, like a facial movement, means a definite attitude of the organism towards certain demands of the environment."

Although we need to know much more about volitional acts and the attitudes to which they are related before a definite program can be planned, it is safe to assume that will, purpose, desire, play important roles. It is from such broad concepts as these that the psychological approach to paraplegia will come. That the intense wish may have its effect at the organic level is a possibility which cannot be ignored. It does not seem altogether impossible that structural changes may be produced in the body in connection with thought processes. Both may be embedded in a larger context and yet a measure of self-initiation allowed; so that to speak of the creative capacity of the mind in connection with the body processes with which it is so intimately related may not be out of order.

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BOOK REVIEWS

CROTHERS, BRONSON, AND PAINE, RICHMOND S. *The Natural History of Cerebral Palsy.* Harvard University Press, Cambridge, 1959. xi + 299 pp., illus. $6.75

In the later part of his life Bronson Crothers, one of the great pioneers in the field of cerebral palsy, elected to re-evaluate the many patients with this disorder that he had seen early in his career. He was able with the help of Dr. Richmond Paine to re-examine or contact by letter some 1400 patients seen personally over a 20-year period at the Children's Medical Center in Boston. The book is the result of the observations of this study and is, in a sense, an attempt "to understand the way in which individual children with cerebral damage grow up under the various types of management."

From a review of their data the authors have managed to accumulate a great many facts which have been detailed relatively well in some 18 chapters. The importance of the book, however, is not in the statistical material which it contains but in that it reflects intelligent thinking about the effect of cerebral palsy on the growth and development of the affected individual. From their rich experience the authors have gained a vivid awareness of the significance of the emotional development of the involved child and the part this factor plays in the eventual attainment of independence and ability to compete in society.

Some authorities may question some of the statements made, *e.g.*, atonic cerebral palsy is not a valid concept, and might disagree with the oversimplified classification of cerebral palsy. Some readers may have difficulty with the table and graphs. Few, however, who have had any great experience with the management of the brain damaged child with motor deficit will disagree with the authors impression "that respect for ingenuity and tolerance to resistance and even rebellion by the child will lead, in many cases, to modification or even abandonment of rigid therapeutic and educational procedures. Certainly there is singularly little evidence that sufficient thought has been devoted to the problems of re-appraising treatment, and perhaps abandoning it as the child grows older."

The Natural History of Cerebral Palsy should be required reading for all those involved in the care of the cerebral palsied individual. Pediatricians and psychiatrists will find in this book more common-sense psychiatry than has been written in the many volumes on psychological medicine.

Sidney Carter

HARTMANN, HEINZ. *Psychoanalysis and Moral Values* (Freud Anniversary Lecture Series, 1959, New York Psychoanalytic Institute). International Unisities Press, New York, 1960. 121 pp. $3.00

Dr. Heinz Hartmann, one of the leading theoreticians of psychoanalysis, prepared this essay as a lecture in the Freud Anniversary Lecture Series sponsored by the New York Psychoanalytic Institute.

As a serious and competent thinker with a masterful grasp of psychoanalysis, Hartmann is always ready to come to grips with the unsolved problems which gather at the borderline between the known and the unknown. Undaunted by the resistance of these to easy resolution, he approaches them directly and introduces as much order as current knowledge and sound thinking will permit. In this essay, Dr. Hartmann's impressive talents are again well employed, and while they do not pretend to dispose definitively of the problem, they at least confront it squarely, leaving the reader with a reliable survey of the field.

The subject, Psychoanalysis and Moral Values, does not define a small, well-knit

area of psychoanalytic thought. Many individual points of contact link these two fields. Hartmann considers some of the most important of these in sequence, and shows both the extent and limitation of current knowledge.

Freud's personal attitude toward ethics and morality must be clearly distinguished from a psychoanalytic one. While Freud accepted the burden of the moral obligations of our society, he stated unequivocally that psychoanalysis could provide no morality of its own and no specific *Weltanschauung*. Many with a personal view which they wish to preach or defend may invoke psychoanalysis in support of their position, but its application to practical problems in human behavior has been worked out only for psychiatric therapy, and to a much lesser extent for pedagogy.

Psychoanalytic theory has managed to elucidate the mode whereby the individual makes the mores and morals of his society effective internal controls. The theory of the superego, its genesis, contents, and mode of operation, all pertain to this problem and invite further investigation. Therapeutic psychoanalysis helps the individual to come to terms with his own morality, both conscious and unconscious portions of it, by disclosing the extent of his compliance and the self-deceptions he employs to resist it. A more specific accomplishment is the investigation of the development of values for the individual. Here Hartmann introduces two original concepts which he has described in previous papers. *Irradiation* of values refers to the fact that the values attached to a given idea tend to irradiate to other ideas associated with the first in a complex. *Agglutination* of values refers to the expectation that ideas with similar values will be found associated in the same complex.

Psychoanalysis itself is not more able than any other science to develop an authentic set of values, a psychoanalytic *Weltanschauung*, so to speak. Hartmann cautions against the substitution of mental health as a value for moral values, and against the substitution of fragments of analytic procedure for social ethics. It is incorrect too, he says, to infer that action in self-interest is more desirable than altruism. The word "rational" is often used as a label to justify behavior which is otherwise questionable from an ethical point of view.

One may ask, as a theoretical question, whether there is such a thing as a biologically given morality, basic to the individual codes specific to each culture. Does therapeutic psychoanalysis tend to foster the adoption, by analysands, of a common moral code? Probably not, say Hartmann, "But what often does happen as a consequence of analysis is that the patient's own authentic moral values become dominant in his codes."

This is the kind of book in which one finds questions rather than answers, but they are essential questions. This reviewer recommends it to all who are seriously interested in the social implications of psychoanalytic theory and practice.

Mortimer Ostow

HINSIE, LELAND E., AND CAMPBELL, ROBERT J. *Psychiatric Dictionary*, 3rd ed. Oxford University Press, New York, 1960. ix + 788 pp. $17.50

In reviewing a dictionary, one finds oneself in an awkward position indeed. To pretend that one has read each reference would tax anyone's credulity beyond measure and to select individual topics for dissent or critique may appear supercilious in the face of a wealth of information which this collection offers.

This is the third edition of the *Psychiatric Dictionary*. The late Dr. Jacob Shatsky has been replaced by Dr. Robert J. Campbell as an editor. The book cover states that 1629 new references have been incorporated. These comprise not only the classical fields of clinical and theoretical psychiatry, but extend into areas of cultural anthropology, experimental psychology and sociology and

the major schools of analytic psychology. Thus, the volume is comprehensive for the student who prefers encyclopedically served information and should also be welcomed by those who like to compare their own bias with the authors' definitions.

Klaus W. Berblinger

BEECHER, HENRY K., ED. *Disease and the Advancement of Basic Science.* Harvard University Press, Cambridge, 1960. xi + 416 pp., illus. $12.50

The underlying theme of the chapters by twenty-one distinguished contributors centers on the kinds of fundamental advances in science which can be made only from the study of disease. Each author elaborates upon this theme from his own viewpoint, drawing upon his own knowledge and experience with frequent historical references. Fundamental concepts are stressed by all of the contributors, who are eminent in their respective fields. A tremendous amount of material has been condensed and each lecture encompasses facts which should be invaluable to the undergraduate student and the graduate as well. The subjects chosen are diverse but all pertinent to the underlying thesis, particularly that in the field of the endocrine system. The other contents include sections on the development of basic concepts in genetics, viruses, cancer, nutrition, anemia, connective tissue and rheumatic disease, the nervous system and mind, and psychiatry. That on cardiovascular physiology, carbohydrate metabolism, homeostatic forces, and a study of subjective responses form an interesting and instructive array of material which is not usually found in a single volume. The chapters entitled "Surgery and Science" and "Surgery, The Amanuensis of Science" will be of particular interest to all physicians. Each chapter closes with a well selected and large bibliography. The reviewer enthusiastically recommends this text not only to those engaged in the field of medical research, but to all students and practitioners of medicine.

Sheldon E. Greisman

DEUTSCH, J. A. *The Structural Basis of Behavior.* University of Chicago Press, Illinois, 1960. ix + 186 pp., illus. $3.50

In the past dozen years a number of authors have attempted to write more or less theoretical accounts of behavior from the standpoint of physiological psychology. Hebb's *The Organization of Behavior* seems to be the example which later writers emulate. The latest book in this tradition is Deutsch's *The Structural Basis of Behavior.*

Deutsch's book fails to live up to its ambitious title. It is, in fact, a rather discursive account of some problems in the study of animal behavior. The problems examined in the book are largely ones about which Deutsch has already published either theoretical or experimental studies. In this book there is some attempt to bring these problems together and to comment on the older and more general literature about the problems examined.

The main theoretical elements used by Deutsch are hypothetical linkages between imagined substructures in stimulus-response networks. These hypothetical linkages, which are rather loosely identified with anatomical entities, provide the common framework by means of which Deutsch treats his variety of problems. The need mechanism, reinforcement and the learning of sequential acts such as those involved in maze learning, are examples of the problems examined by Deutsch.

Like most such theories, the hypothetical constructs, upon which the theory is based, are designed to explain behavior data not physiological data. As a means of analyzing behavior, Deutsch's notions seem to have no special merit, nor do they lead him to tackle novel or unusual problems. Therefore, the book will appear to be important and useful

to the extent that one regards the study of some of the individual problems to be a fruitful enterprise.

The book itself is uneven. Students of behavior theory will no doubt find that Deutsch does have some interesting ideas about certain specific problems (for example, the ideas about insight and latent learning), but it is very unlikely that the general psychological reader will persevere through every page of the book. It is, in short, the sort of book that it is worthwhile to leaf through, look at the diagrams and read in detail any section that strikes one's fancy as being relevant, interesting or important.

James Deese

MASSERMAN, JULES H., ED. *Science and Psychoanalysis, Volume II: Individual and Familial Dynamics.* Grune and Stratton, New York, 1959. vi + 218 pp. $6.75

Historians may well look back on the last decade as the *Age of Symposia.* This rapidly growing phenomenon poses a new problem for the book reviewer. No longer can one comment on an author's ideas from the standpoint of "the critic." The reviewer finds that the "board of panelists" has subsumed this role. What is left for fresh discussion after ten or fifteen panelists have commented on the original paper is often so far to the periphery, and so far into the abstract, that the reviewer can do nothing else but use as his subject matter the *broad philosophical connotations* and the *world trends* made manifest by the vast numbers of people who have already commented and recommented on comments. However, our purpose is not to declare that the symposium is a detriment to the psychiatric field. Certainly, there are some papers of considerable interest in the present volume.

The book is divided into two parts. It is a collection of papers and comments delivered at two symposia of the Academy of Psychoanalysis, the first being held in December, 1957, and the second in May, 1958. The first section is entitled *Masochism*; the second is called *Familial and Social Dynamics.* The section on masochism begins with a paper by Dr. Leon Salzman. It is concerned with the historical derivatives of the term, and more recent concepts of the psychodynamics and treatment of patients who exhibit this rather vaguely defined behavior. The historical perspective is of interest and very readable. He concludes that the therapist's role must be flexible enough to allow him to confront the patient with the reaction her behavior has on the therapist. This is rather vague, but becomes the point around which some disagreement arises as the following speakers comment on Dr. Salzman's paper.

Dr. Harold Kelman discusses masochism as it is seen by Horney. He feels that the masochistic behavior of Dr. Salzman's patient might better be expressed, theoretically, as the need for "self extinction." Dr. Clara Thompson discusses the subject from the interpersonal viewpoint. Drs. May Romm, Paul Hoch, John A. P. Millet and William V. Silverburg follow Dr. Kelman, and discuss the great variability of the kinds of people who are masochistic, the presence of guilt and sexual perversion within the psychodynamics of such patients, and some aspects of the therapeutic role. Dr. Masserman, the author of the next article, begins with the question of whether or not there is, in fact, such a thing as masochism, and whether it is a useful category to discuss. In a quite refreshing style, Dr. Masserman states the thesis that patients really seek suffering in order to accomplish certain well-defined goals, goals which may be conscious, preconscious, or unconscious. He goes on to discuss the therapeutic role of the doctor in terms of whether or not it is realistic to maintain a studied aloofness and the "delusion" that the patient is all-suffering, and not a manipulator of social events.

As the first section of the book closes,

there is a growing freshness, as the discussion of the appropriate therapeutic role becomes more free. The concept of masochism itself seems to be discussed with more openness and less dogma.

The second section of the book, *Familial and Social Dynamics*, begins with a paper by Dr. Grotjahn. This paper is a very refreshing look at psychoanalytic theory, including its shortcomings as well as its major contribution to our knowledge concerning family dynamics. Dr. Grotjahn points out that the historical perspective of a patient's neurotic behavior is not sufficient and should be supplemented with a present-day perspective, including a view of the sanctions and modes of communication used by the family. This paper is followed by one on the psychoanalytic approach to the family by Dr. Nathan Ackerman. Dr. Don Jackson next discusses "family homeostasis" and several interesting categories of family dynamics which seem to be relevant to the problems of prognosis and treatment. Basic to Dr. Jackson's orientation is the searching out of the fundamental modes of communication within the family constellation. This interest in communication is combined with an interest in family values and *role playing* within the family. Drs. Fleck and Lidz, *et al.*, discuss what the authors feel to be a rather common characteristic of families in which schizophrenia has occurred, namely, an incestuous and/or homosexual component. Several patients are discussed who demonstrate strong evidence that this was a stressful factor in their experience. The next paper, by Dr. John Spiegel, is of considerable interest and of great relevance to the psychotherapist. It concerns the effect of the cultural norms of both the patient and the therapist on the final outcome of the treatment. Dr. Spiegel attacks the rigidity with which the therapist very often sees his role. The need to be "alive" with patients is discussed, particularly those patients whose cultural background calls for a high degree of affective interchange in social relationships. Dr. Spiegel further points out that many middle-class American therapists expect, without realizing that they do so, that the patient be interested in his future, be a hard working person in therapy, be interested in growing independence for himself, and be interested in looking for natural causes within himself rather than satisfied that fate is the source of much of his behavior. He points out that these expectations may not coincide at all with many other cultural backgrounds. A therapist who cannot adjust his own goals and expectations so that he can form a positive relationship with the patient may find that his therapy is unsuccessful. We might add that the patients who are unfortunate enough to be involved in such unsuccessful therapy trials are commonly known as "poorly motivated."

The last paper is by Dr. Alexander Gralnick and is concerned with family psychotherapy.

Though many of the articles are not of great interest, the book moves with sufficient continuity and contains a sufficient number of noteworthy papers to be of interest to any person who is concerned with the theory and practice of psychiatry. Overall, there seems to be a striking interest in expanding theoretical frameworks, particularly in the area of group and family dynamics. There seems also to be an interest in evaluating and reconstructing appropriate therapeutic roles, which have too often been conceived as relatively fixed, without regard for the differences between patients. The trend in this area seems to point in two directions. On the one hand, "conjoint therapy," or therapy with family members and the patient in group sessions, seems to be a strong trend. In another direction, there seems to be a growing interest among some of the authors toward a more "alive," more flexible role for the therapist, even in individual psychotherapy. It seems to me that this growing flexibility is a good thing and should be followed by more research in this area.

The present symposium is not altogether

free of repetitious and dogmatic comment, which at times seems to imply that it is almost a sin to think that all is not known about such a vague entity, for example, as masochism. Along side these comments, however, are others which imply an energetic interest in expanding our present theories and in further development in the newer areas, such as communication and learning theory, along with an increased rigor in method development.

Sheppard G. Kellam

COLBY, KENNETH MARK. *An Introduction to Psychoanalytic Research.* Basic Books, New York, 1960. ix + 117 pp., illus. $3.00

This little book deserves a big review; Dr. Colby has again demonstrated that he is a particularly thoughtful and conscientious psychoanalyst with an unusual breadth of interest. He has, moreover, a facility for discussing important issues in an entertaining and concise manner. As an introduction to the application of scientific method in the psychoanalytic situation, the book will be useful to the neophyte in the behavioral sciences, yet valuable to the sophisticate as a reminder of his often forgotten, twentieth century metaphysical heritage. In the first twenty pages the author defines terms, traces the history of scientific methodology, and discusses the contemporary attitude on certainty, relativity theory, language, assumptions, and structure of scientific theory. Such compression indicates the introductory quality of the book but should not imply superficiality, as Dr. Colby has documented most of his statements with an annotated bibliography of unusual depth. This adds immeasurably to the value of the book as an outline for further reading. References to scientific method are represented by Feigl, Woodger, Goedel, Wiener, Bronowski, and Rescher; to physiology and psychology by Kleitman, Magoun, McLean, Heider; to cultural anthropology and communications by Kluckhohn and Weaver, to mention only a few.

The author defines a scientist as "a person who makes observations and/or experiments to obtain data relevant to a hypothesis about a problem in a selected portion of the world of experience in order to decide whether or not the hypothesis is correct." He then goes on to discuss "methods appropriate to our data and problems." Within the framework of experimental science he describes the techniques particularly useful to the psychoanalyst. As an example, he states, "Introspection, once disparaged as unreliably subjective, is now beginning to be recognized even by the operationalists as a type of observation and hence a legitimate source of data. . . . Searching for regularities in these data one formulates principles giving a causal explanation of them." One can see that the author's point of view will satisfy neither the "objective" scientist nor the "introspectionist." Dr. Colby recognizes this, and in his several "fables" humorously disparages these attitudes.

On the other hand, he convincingly challenges other students of human behavior to devise a better setting than the psychoanalytic one for the scientific study of human behavior. The psychoanalytic setting is an extensively detailed recording of an individual's behavior in the broadest sense over an extended period of time; this is accomplished within well-defined limits; the situation is relatively constant from day to day and uniquely stable in its interpersonal relationship; inferences from the data can be repeatedly checked against the patient's behavior and the therapist's introspection; the method of observation is less likely to interfere with the natural process than any heretofore devised experimental setting; and independent variables are introduced into the controlled setting in the form of therapeutic intervention. This is by no means a naive evaluation of the therapeutic situation: the unique complications of such ob-

servation are lucidly discussed in a section on "communicant observation."

In the course of his book Dr. Colby good naturedly steps on everybody's toes, but wholeheartedly sides with those critics of psychoanalysis who emphasize the uncontrolled and casually recorded observations of the analytic case history. He objects to the lack of quantification and experiment as well as the absence of controls and the paucity of follow-up studies in most psychoanalytic research. He recognizes the frequent obscurantism of language and a preoccupation with pseudo-questions or unanswerable ones (*e.g.*, philogenetic memories) and an all-too-frequent recourse to pseudo-explanations (*i.e.*, describing the same phenomena with two different sets of verbal terms). He does not, however, believe that these criticisms represent inherent deficiencies in psychoanalysis as a science but rather the shortcomings of the psychoanalyst as a scientist. The author's suggestion is that these deficiencies can be overcome by sharpening our observational methods through the use of more complete and objective recording techniques, *e.g.*, tapes and films; that we enhance accuracy of observations by multiple observers and apply more rigorous statistical techniques to the treatment of data, particularly small group statistics.

Perhaps the most serious question that could be raised about this book is whether the value the author places on the statistical statements and units of measurement is justified. Do psychoanalytic data lend themselves appropriately to this type of treatment? The reviewer regrets he is not competent to adjudge such statements as the following: "Our example illustrating one use of probability theory in psychoanalysis has involved the Neyman-Pearson interpretation of probability which utilizes a null hypothesis, one or more alternative hypotheses, a choice of level of significance, and the selection of a critical set of results which leads to rejection of the null hypothesis." Perhaps the book will serve the author's purpose if it encourages the reviewer and others to become more competent in this area. In all fairness it must be stated that Dr. Colby recognizes the dangers in being inappropriately "scientific"—at one point he remarks, "But again we should not be so rigorous as to induce rigor mortis." He justifies his proposed experimental model as follows. "The physics model has limitations in biological science, especially in the behavioral sciences. We will have to make another transformation in scientific method and develop new approaches more appropriate to our subject matter . . . however, since we do not as yet have the new transformation in scientific method, we should try to exhaust the possibility of the current procedural model." Few would argue against the latter point, but how can the author be so sure that approaches already proposed, such as those of the "finalist" or the "phenomenologist," do not possess a new transformation for which the behavioral scientists are looking?

Russell R. Monroe

STEIN, MORRIS I. AND HEINZE, SHIRLEY J. *Creativity and the Individual.* Free Press, Glencoe, Illinois, 1960. viii + 428 pp. $10.00

Creativity, its definition, origins, stimulation and application, has been widely studied with a considerable divergence of opinion resulting. Stein, an Associate Professor of Psychology, and Heinze, a Research Associate, both at the University of Chicago, have compiled this survey of the field.

In summarizing over three hundred books and articles related to the subject of creativity (written since 1900), the authors have not attempted to make an original contribution. They state their purpose is to be as objective as possible. Their "straight-away" reporting with an almost complete absence of editorial comment supports this aim.

Dividing their book into thirteen sections classified as: Criteria, Creative Process, Heredity, the Nervous System, Age, etc., the

authors present a rather distinct impression of what has been done in the study of Creativity. The summaries of the various works referred to are written in a concise and clearly understandable fashion. The style of presentation is sufficiently varied so that one may review, consecutively, a considerable number of papers without developing a feeling of monotony. Spot-checks of the summaries with the original works reveal a reasonable accuracy in reporting, considering the obvious space limitations.

Creativity and The Individual is not quite the "ideal reference volume" which the editor describes it to be, due to the absence of separate author and title indexes. Authors of summarized works are included in the general index along with the names of men listed within the papers and there is no separate listing of titles. This may leave the harassed scholar thumbing the pages of the book, trying to remember or to guess the sections under which the authors have chosen to place a particular summary.

On the whole, however, the book is quite stimulating and should prove to be of considerable interest to anyone who has a desire for a digested version of the large amount of both experimental and theoretical work published on this subject.

Richard H. Phillips

KRETSCHMER, ERNST. *Hysteria, Reflex and Instinct.* Philosophical Library, New York, 1960. xi + 162 pp. $4.75

Professor Kretschmer was kind enough to let me know that the first German edition of this book was called *Ueber Hysterie* and published in English in 1926 under the title *Hysteria.* This book is presumably a translation of the sixth pertinent German publication (1958). Kretschmer explicates that certain hysterical patterns are closely related to the drive of preservation and to the sex drive, and that there might be vegetative and/or endocrine factors at work. He distinguishes social from endogenous hysterias. Quite a few hysterical manifestations, he avers, can be understood as a more or less voluntary increase of reflexes. He discusses the "hypobulic will" as "capable of functioning as an independent component within the overall sphere of expression." The hypobulic type of will is "the ontogenetic and phylogenetic lower stage of the purposive will." We are told "The hysterical reaction . . . bears the same relation to the normal reaction as does instinct to intellect."

This book, in which Kretschmer refers to Kraepelin as well as to Freud, does not present anything essentially new. Many readers may indeed think that much of it sounds unessential or old, though presented in the frame of the author's *Medical Psychology.* Yet it captivates through the elegance of the author's presentation and his fortuitous gift for expression, *e.g.*, "Yesterday's normal instinct can become tomorrow's neurosis."

This review gives the writer once more the opportunity to express his regret that Kretschmer's masterpieces—"Bodybuilt and Character," and "The Paranoia of the Sensitive"—are not better known in this country.

Eugen Kahn

WILLIAMS, ROGER J. *Alcoholism: The Nutritional Approach.* University of Texas Press, Austin, 1959. x + 118 pp. $2.50

Roger J. Williams, Professor of Chemistry and Director of the Clayton Foundation Biochemical Institute at the University of Texas, has for years been deeply interested in alcoholism and has studied this illness from the point of view of metabolism and nutrition. It is his opinion "that the disease of alcoholism is essentially a disease of one's appetite and insofar as this is true it probably can be consistently prevented by the application of nutritional knowledge. The role of individuality in body chemistry must be recognized, as well as the fact that no individual person can be sure, in advance, that

he does not have the potentiality of becoming an alcoholic."

Williams points out at great length the marked differences in individual needs for nutrition. He speaks of biochemical individuality and documents this by impressive evidence, and he concludes that biochemical individuality is a factor in the problem of alcoholism. He has prepared a prescription for nutritional supplementation of the alcoholic, including various vitamins, minerals, lipocaic acid and glutamine.

Originally, it had been the hope that such supplementation would enable individuals suffering from alcoholism to drink in a controlled, non-problem manner, but experience did not confirm this and it became necessary to modify this expectation to advocate complete abstinence along with nutritional supplementation.

The current position held by Williams is that "as long as nutrition is seriously impaired, the individual alcoholic is seriously handicapped in his utilization of psychological and spiritual influences in bringing about his rehabilitation."

The diet of the alcoholic, according to Williams, should contain plenty of high quality protein, certain unsaturated fat acids and should avoid refined foods such as white rice, spaghettis, etc.

The search for the etiology of alcoholism still needs to continue. There are those who emphasize the psychological and cultural factors and those, like Williams, who emphasize the nutritional, metabolic and biochemical factors. It is quite possible that a combination of these factors are operative. Of course, the hope of the alcoholic is that there may be discovered some "simple medicine" which will enable him to drink again in a pleasurable manner.

Alcoholics who fairly routinely start out by trying to learn how to drink, rather than how to achieve and maintain sobriety, need to experience significant shifts in attitudes and often a modification in the balance of forces within their family before they can sincerely and steadfastly value their sobriety and doggedly retain it. That has happened in a sufficient number of cases where no specific attention was given to dietary regime to emphasize the outstanding importance of the emotional and psychological orientation of the individual alcoholic. Perplexity still exists by the choice certain individuals make of alcoholism as a way of meeting their life problems, and the absence of this "pathological solution" in others who have quite similar emotional difficulties.

It is also difficult to understand, on an entirely psychological basis, why individuals who have maintained their sobriety for years and have functioned in a reasonably normal way, revert to serious drinking difficulties when they venture again to experiment with social drinking. Williams certainly has opened up an important area for further investigation when he refers to his efforts to discover the "alcoholic prone" individual. Such early case finding would be invaluable in terms of preventive medicine. Also, continued investigation of the special biochemical characteristics and differentiated features of the alcoholic should be strikingly useful in the management of this serious condition.

The tragic fact remains, that of the great number of alcoholics in this country, relatively few are able to win their way to sustained sobriety with the present techniques and resources available.

The "nutritional approach" is an important broadening of our understanding of this wide-spread condition.

Isadore Tuerk

MOWRER, O. HOBART. *Learning Theory and Behavior*. Wiley, New York, 1960. xii + 555 pp. $6.95

O. Hobart Mowrer presents "revised two-factor theory" in *Learning Theory and Behavior*. Mowrer believes that this position is the one toward which recent developments in the field of psychology as a whole are

pointing and that it approaches more nearly a general behavior theory than do earlier learning theories.

The core of revised two-factor theory is presented in the seventh chapter, the preceding six chapters tracing an historically oriented argument concerning alleged inadequacies of alternative positions. This core chapter is followed by five chapters that amplify upon the theory in a variety of contexts such as generalization and discrimination.

First, it must be said that reading the book is, in many of its sections, extremely tedious. Numerous quotations from other writers and from Mowrer's mimeographed class notes are sandwiched between transition paragraphs and a number of long footnotes. The difficulty imposed upon the reader by the fact that almost half of the book is in fine print must at least share the responsibility for any subsequent remarks in this review that reflect a basic misunderstanding of Mowrer's position.

This criticism is not trivial. For example, in one section Mowrer traces the generally negative results of attempts to obtain secondary reinforcement based on the termination of a noxious stimulus. Revised two-factor theory virtually demands that such results be positive and the text discounts the previous studies on procedural grounds. A footnote then updates this argument by referring to Crowder's dissertation which Mowrer describes as providing, for the first time, unambiguously positive results. Then, after referring to an earlier study, Mowrer concludes that one may, with considerable confidence, accept the validity of the discussion in the text. This statement of confidence contrasts sharply with Crowder's own conclusion that his studies provide only weak evidence for secondary reinforcement based on shock termination. This caution was justified because only one of four experiments yielded statistically reliable results that could be interpreted as indicating secondary reinforcement, and this finding is subject to a different interpretation not involving secondary reinforcement. Hence, the unwary reader will be grossly misled by the style with which Mowrer displays his own convictions.

Indeed, the need for caution in reading *Learning Theory and Behavior* is quite general. Mowrer's account of recent history and his discussion of contemporary theories are frequently highly controversial. Many will disagree with his chronology of events, and exception could legitimately be taken to virtually every instance in which Mowrer attempts to discount Hull's theory.

Of course, judgment must rest primarily upon the adequacy of revised two-factor theory itself. In extremely shortened form, this position assumes that organisms attempt to maintain contact with stimuli that have in the past been associated with drive reduction (and hence produce "hope" that something good is about to occur, or "relief" that something bad is about to cease), and to escape contact with stimuli that have in the past been associated with drive induction (and hence produce "fear" that something bad is about to occur or "disappointment" that something good is about to cease). In this conception, instrumental behavior is not determined by habit-bonds between stimuli and overt responses but instead is guided cybernetically as the organism continues to perform in such a way as to maintain response-produced stimuli associated with hope and relief and stops performing in such a way as to maintain response-produced stimuli associated with fear and disappointment.

There are several comments about this position that seem appropriate. First the theory is not yet very well developed in the sense of providing explicit anchoring of the theoretical constructs. New terms, such as stimulus trace, fatigue, etc., are introduced quite informally in the later chapters and there are few rigorous statements relating to such common dependent variables as speed, amplitude, or rate of change, or to

such common independent variables as the number of trials or the amount of reward. In a similar vein, noncircular definitions of drive-reductions and drive-inductions are not offered.

Furthermore, this type of theory would appear to have difficulty accounting for short, rapid responses. Consider, for example, the effects of delayed auditory feedback on human speech. If the speech of a person who is reading aloud is recorded and played back to him through earphones after about a quarter-second delay, his speech is typically distorted. In conventional S-R theory, feedback cues from one response serve in the selection of the next response, and interfering with the former will disrupt the latter. In revised two-factor theory, however, feedback cues serve in the selection of the response that is now occurring. Certainly, the selection of a sound cannot be guided by feedback from the overt response itself since the action would have to work backward in time upon something that has already happened. Yet if the selection is made in some ideational center where the possible sounds are scanned before they occur, then interfering with feedback in the overt system need not necessarily disrupt this selection process. In any event, many would disagree with Mowrer's assertion that the effects of delayed auditory feedback are incomprehensible to traditional S-R interpretations of habit.

A final observation about the type of theory described by Mowrer is that instrumentally adaptive behavior should be impossible in the absence of feedback. Neal E. Miller, in describing his visit to Konorski's laboratory in Warsaw, reports that scientists there have obtained both learning and performance of an instrumental act even after the limb involved in the response had been completely denervated. Hence, the Polish neurophysiologist whom Mowrer refers to in various other contexts has been forced to modify his own conception of instrumental behavior as being completely mediated via feedback.

Notwithstanding the preceding criticisms, *Learning Theory and Behavior* is a significant contribution to the field. True, this reviewer found some of the expositional features inaccurate and tendentious, and believes that there are already enough data available to show that revised two-factor theory is not an adequate behavior theory. Mowrer has indeed adopted several lines of thought that have been becoming increasingly the vogue, and he has synthesized these creatively into the seed of a definitive theory. Serious students of behavior will certainly want to reckon with it.

Frank A. Logan